OBITUARY NOTICES

OF

FELLOWS DECEASED.

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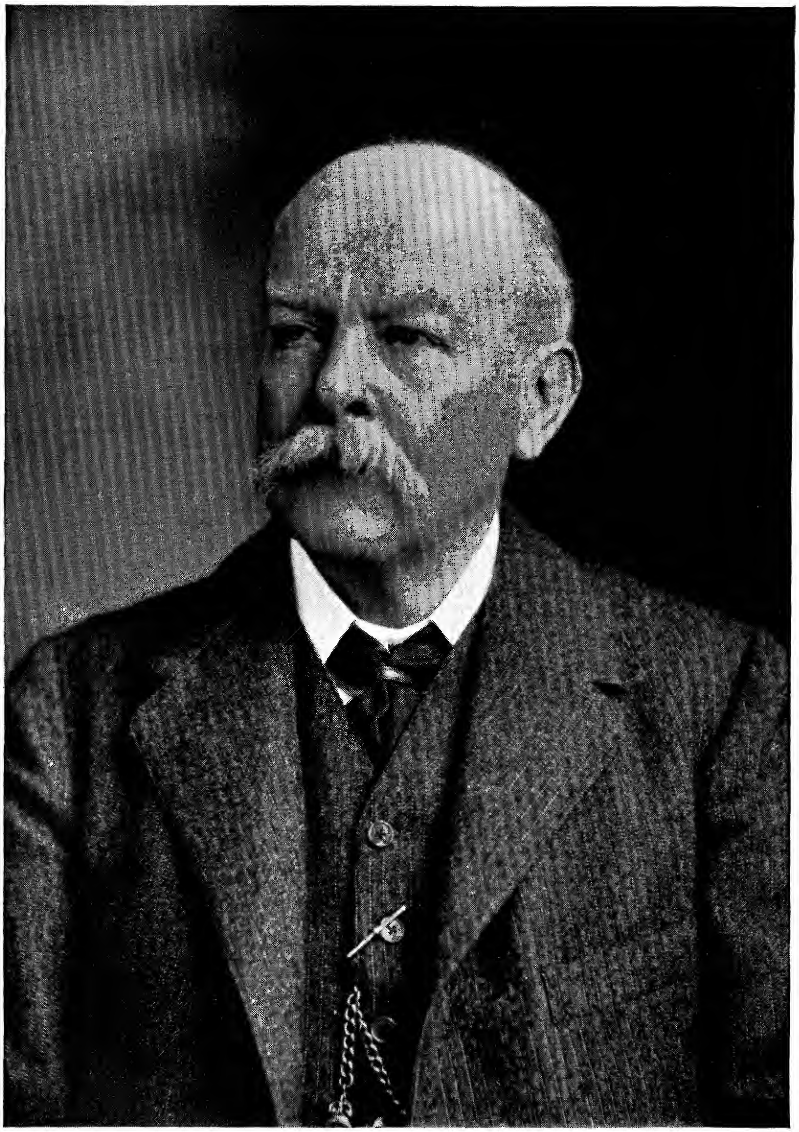


J. H. POYNTING

JOHN HENRY POYNTING, 1852–1914.

John Henry Poynting, the second son of the Rev. T. E. Poynting, Unitarian Minister at Monton, near Manchester, was born there on September 9, 1852. He received his earlier education at the school kept by his father and then went, in 1867, to the Owens College, which his elder brother, C. T. Poynting, who was for many years Unitarian Minister at Fallowfield, near Manchester, had just left. Poynting must have received a good grounding in Mathematics at his father's school, as he gained a Dalton Entrance Exhibition in Mathematics before entering the College. Owens College in those days was in a modest building, once the residence of Richard Cobden, in Quay Street, Deansgate. Neither the amenities of the locality nor the accommodation in the building were anything to boast about, but few educational institutions before or since, whatever their equipment or surroundings, have had a more efficient staff than Owens College in the old Quay Street days. As an old Quay Streeter, the writer can speak from personal experience. The cramped space was not an unmixed disadvantage. We were so closely packed that it was very easy for us to get to know each other, Arts students and Science students jostled against each other continually; a crowd of Mathematicians would be waiting outside the doors of a lecture room for it to discharge a Latin or Greek class, and thus one of the chief difficulties of non-residential colleges, the lack of social intercourse between the students, was almost absent.

The professors at Owens in Poynting's time were: Barker for Mathematics, of whom Poynting always spoke in terms of the highest appreciation, a feeling shared by all his pupils, for no abler or more conscientious teacher of Mathematics than Thomas Barker ever lived; Jack, another great teacher, was Professor of Natural Philosophy; Roscoe of Chemistry, and Williamson of Natural History; on the literary side, Greenwood, the Principal, was Professor of Classics, Ward of English History and Literature, Jevons of Logic, and that very lovable man, Theodores, lecturer on Modern Languages. At that time Owens had not the power of granting degrees, and most of the students prepared for the examinations of the University of London. In those days these covered a very wide range of subjects, and Poynting, who took the London degree, must have attended the lectures of all these professors. He was second at the London Matriculation in 1869, obtained Second Class Honours in both Physics and Mathematics in the First B.Sc. examination in 1871, and took the B.Sc. degree in 1872. In the spring of 1872 he obtained an entrance scholarship at Trinity College, Cambridge, and came into residence at Cambridge in October. At Cambridge he pursued the normal course of one destined for high honours in the Mathematical Tripos. He read with Routh, he obtained his Major Scholarship in due course, like many of the reading men of his time at Trinity he joined the Second Trinity Boat Club and rowed in the first boat in 1875; the fortunes of that once famous club were, however, then declining and it came to an end in 1876. He took his degree in the Mathematical Tripos of 1876 as Third Wrangler, bracketed with Mr. Trimmer, of Trinity College, a very brilliant man who suffered from persistent ill-health and died within a few months of taking his degree. As Dr. Glazebrook and Dr. Shaw both graduated in the same Tripos and Lord Rayleigh was the additional Examiner, Physics was well represented on this occasion.

After taking his degree Poynting came back for a short time to the Owens College, which was now in the buildings it at present occupies, and demonstrated in the Physical Laboratory under Prof. Balfour Stewart, who had succeeded Jack as Professor of Natural Philosophy shortly before Poynting's departure for Cambridge.

On his election to a Fellowship at Trinity College in 1878, Poynting returned to Cambridge and began, in the Cavendish Laboratory under Clerk Maxwell, those experiments on the mean density of the earth which were destined to occupy so much of his time for the next 10 years.

He remained at Cambridge until 1880, when he was elected to the Chair of Physics in Mason College, Birmingham (now the University of Birmingham), which had just been founded; this post he held until his death. The year that he went to Birmingham, he married the daughter of the late Rev. J. Cropper, of Stand, near Manchester.

He threw himself whole-heartedly into the arduous duties connected with the starting of a new University College, the preparation of his lectures and the equipment of the physical laboratory, and, as was his wont, without any bustle or hurry he soon had things working efficiently. And so in the efficient discharge of his duties as a Professor, in successful original research, in the fulfilment of municipal duties, the time passed placidly on, the only cloud on an almost idyllic domestic life being his somewhat indifferent health, the first threatenings of the disease from which he ultimately died. To see if a country life would suit his health better than a town one, the Poyntings moved from Edgbaston to Fox Hill, Alvechurch, a house about 12 miles out of Birmingham. There was a small farm attached to the house and Poynting entered into farming most heartily, though I am afraid he did not derive much pecuniary profit from it. But even farming when the agricultural depression was most acute could not impair his good temper or ruffle his equanimity. If the farm did not yield money, it gave new interests and experiences, and if something was always going wrong, at any rate it drove away monotony. The quietness and simplicity of the life were thoroughly to the taste of Mrs. Poynting and himself. Life in the country too gave free scope to his taste for Natural History, in which he always took great interest; he was a keen and excellent observer, and a favourite contention of his was that physicists were somewhat too much inclined to confine their observations to experiments made in the laboratory and did not sufficiently avail themselves of the opportunities of studying the physical phenomena going on in the sky, the sea, and the earth. The taste for Natural

History was a family one; his younger brother, the late Mr. F. Poynting, was an excellent ornithologist, devoting himself especially to the study of the eggs of British birds, of which he made most careful and accurate water-colour drawings—some of these have been reproduced in his book 'The Eggs of British Birds.'

The Poyntings stayed at Foxhill until 1901, when, his health much improved, they returned to Edgbaston. His life at this time was a busy one, for in addition to the work demanded from him as the head of a large and successful School of Physics, he acted as the Dean of the Faculty of Science, was a Justice of the Peace, and for some time Chairman of the Birmingham Horticultural Society. He had also to plan and superintend the erection of a new physical laboratory when his department was transferred from its old quarters to the new buildings of the University of Birmingham. He went with the British Association to Canada in 1907, when it met at Winnipeg, and gave one of the evening lectures; his subject was the Pressure of Light, on which he had been experimenting for several years. He went the trip to Vancouver and back and seemed thoroughly to enjoy the visit. The pressure of light was also the subject of a lecture which he gave in French at Paris before the French Physical Society at Easter 1911.

In the spring of 1912 a severe attack of influenza was followed by a recrudescence of diabetes, a disease from which he had suffered for some time, and he was ordered to take a long rest; he was, in consequence, away from Birmingham for two terms. On his return to Birmingham he seemed much better, he took an active part in the meeting of the British Association held there in September, 1913, and he and Mrs. Poynting entertained a large party of physicists at their house in Ampton Road, and it then seemed as if he might hope to enjoy many years of useful work. Another attack of influenza in the spring of 1914 brought on a very severe attack of diabetes, and he died on March 30, 1914.

It is difficult to attempt to say what Poynting was to his friends without using terms which must appear exaggerated to those who did not know him. He had a genius for friendship, and a sympathy so delicate and acute that whether you were well or ill, in high spirits or low, his presence was a comfort and a delight. During a friendship which lasted for more than thirty years, I never saw him angry or impatient and never heard him say a bitter or unkind thing about man, woman or child.

He took pleasure in many things, in music, in literature, for he was a lover of books and a collector in a modest way, in novels of all kinds, good and bad. He was fond of the country, and especially of North Wales, where he spent most of his vacations, but happiest of all when at home with his family. Throughout his life he took considerable interest in Philosophy, and a discussion of the philosophical basis of Physics formed part of his Presidential Address to Section A at the Dover Meeting of the British Association. Views similar to those he there expressed are now held by many; he had formed his years before, when but few in this country agreed with them. The excellence of his work received many recognitions, though not in my opinion so many as it deserved. He was elected a Fellow of the Royal Society in 1888, received a Royal Medal in 1905, served on the Council from 1909 to 1911 and was Vice-President in 1910–11. He received the Adams Prize from the University of Cambridge in 1893, the Hopkins Prize from the Cambridge Philosophical Society in 1903. He was President of Section A when the British Association met at Dover in 1899 and was President of the Physical Society in 1909–11.

He was in great request as an Examiner in Physics and no one excelled him at this work, his long experience of students, his judgment and common sense, the charitable view he took of the limitations of a student's knowledge, and the fact that he was never afraid of setting easy papers, made him an eminently fair and discriminating examiner. He was very successful as a teacher of students of all kinds, those who only took Physics as a subsidiary subject as well as those who made it their life's work, he inspired these with an enthusiasm for research and with some of his own skill in accuracy of measurement and in the thoroughness of their work.

POYNTING'S SCIENTIFIC WORKS.

This may be divided into four groups: (*a*) studies on gravitational attraction, (*b*) on the change of state, (*c*) on the transfer of energy in the electromagnetic field, and (*d*) on the pressure of light.

Gravitational Attraction.

His experiments on the mean density of the earth were commenced in Cambridge in 1878 but it took twelve years' steady work before he obtained a result with which he was satisfied. The method used was to measure the attraction between two known masses A and B by suspending A from one of the arms of a balance of the ordinary type and finding the increase in weight produced when B was brought underneath it. The balance used in the later experiments was one built specially for the experiment by Oertling and had a beam 123 cm. long. With a balance of this size the difficulties arising from air currents proved very formidable. Poynting fully recognised the advantage of Boys' short torsion balance method in this respect and said that if he were designing the apparatus again, instead of using an exceptionally large balance for the sake of being able to suspend large masses, he should go to the other extreme and make the apparatus as small as possible. At the same time, as he points out, the magnitude of the effects produced by the air currents made their detection easy, whereas they might have been overlooked and not allowed for had they been smaller. The final results ('Phil. Trans.,' A, vol. 182, p. 565, 1891) he obtained for Δ, the mean density of the earth, and G, the gravitational constant, were

$$\Delta = 5{\cdot}4934.$$
$$G = 6{\cdot}6984 \times 10^{-8}.$$

Poynting's long investigation incidentally added considerably to our knowledge of the technique of accurate weighings.

With the co-operation of Gray he made a series of most interesting experiments ('Phil. Trans.,' A, vol. 192, p. 245, 1899) to see if the attraction between two quartz crystals was the same when the axes of the crystals were parallel as when they were crossed. The method he used was a very ingenious application of the principle of forced oscillations, which was so effective that, though one sphere was only about 1 cm. in diameter and the other about 6 cm., the experiments showed that the attractions in the two positions could not differ by as much as 1 part in 10,000. Later he made with Phillips a series of experiments to see if weight depended on temperature, using as in his first experiments a balance of the ordinary type; the result of these was ('Proc. Roy. Soc.,' A, vol. 76, p. 445, 1905) that between 15° C. and 100° C. the change in weight is not greater than 1 in 10^9 and between 16·6° C. and −186° C. it is not so great as 1 in 10^{10} per 1° C.

Change of State.

The problem of the change of state was one in which he took especial interest, and it was the subject of one of his earliest papers ('Phil. Mag.,' (5), vol. 11, p. 32, 1887). His way of picturing this change was to suppose that from the surface of a liquid or solid particles were continually breaking free, so that through each unit of area of the surface there was a constant escape of molecules. This loss was balanced by the passage from the vapour above the solid of some of the gaseous particles which struck against its surface, so that when there was equilibrium the flow out from the liquid or solid was balanced by the flow inward from the gas. The proportion of gaseous molecules which after striking the surface passed across to the solid or liquid state he assumed to be the same for a solid as for a liquid and to be independent of the temperature, so that it could be measured by the vapour pressure. Thus at the same temperature the flow across water would be proportional to the vapour pressure of water, that across ice to the vapour pressure of ice, thus ice could only be in equilibrium with water when the vapour pressure over ice is equal to that over water.

Poynting supposed that the mobility of the molecules in liquids and solids is increased by pressure—the pressure as it were squeezing the molecules out: the amount of the increase depending on the density of the substance, diminishing as the density increases. Thus, if pressure increases the escape of the molecules from a liquid, a liquid under pressure will evaporate more freely, and so for it to be in equilibrium with its vapour the vapour pressure must be higher than that over the normal liquid; from the equilibrium between water and its vapour in a capillary tube, he found that if δp is the increase in the vapour pressure produced by applying a pressure P to the liquid, $\delta p = \mathrm{P}\rho/\sigma$, where ρ is the density of the vapour and σ that of the liquid.

Poynting applied this conception of mobility to the case of solutions, taking the view that the molecules of the salt formed aggregates with some of the water molecules and thus diminished their mobility thereby diminishing the number of water molecules which passed from the liquid state through each unit of area of surface per second. The mobility of pure water is thus greater than that of the solution, so that if the two are separated by a semi-permeable membrane more molecules will pass from the water to the solution than from the solution to the water, and the water will flow into the solution. To prevent this flow the mobility of the molecules of water in the solution must be increased by the application of a pressure that will make the mobility of the solution equal to that of pure water; this pressure is the osmotic pressure. Since under this pressure the mobility of the solution is equal to that of pure water the vapour pressure in equilibrium with the pressed solution will be the vapour pressure over pure water, so that another definition of osmotic pressure would be the pressure required to raise the vapour pressure over the solution to that over pure water. On the assumption that the presence of one molecule of salt to n of water would diminish the mobility of the water in the proportion of $(n-1)/n$, which would be the case if a molecule of salt imprisoned one and only one molecule of water, Poynting showed that the osmotic pressure on his theory would be the pressure exerted by the salt molecules if they were in the gaseous state and occupying the volume of the solution. Though this theory does not connect the electrical properties of solutions with the properties associated with osmotic pressure so readily as the dissociation theory, it is so simple and fundamental that it helps to give vividness and definiteness to our picture of the processes operative in solutions.

Transfer of Energy.

The researches by which Poynting is most widely known are those published in the papers "On the Transfer of Energy in the Electromagnetic Field" ('Phil. Trans.,' A, 1884) and "On Electric Currents and the Electric and Magnetic Induction in the Surrounding Field" ('Phil. Trans.,' A, 1888). He says in the first paper, "The aim of this paper is to prove that there is a general law for the transfer of energy, according to which it moves at any point perpendicularly to the plane containing the lines of electric and magnetic force, and that the amount crossing unit of area per second of this plane is equal to the product of the two forces multiplied by the sine of the angle between them divided by 4π, while the direction of the flow of energy is that in which a right-handed screw would move if turned round from the positive direction of the electromotive to the positive direction of the magnetic intensity." He shows from the equation of the electromagnetic field that the rate of increase in the energy inside a closed surface is equal to

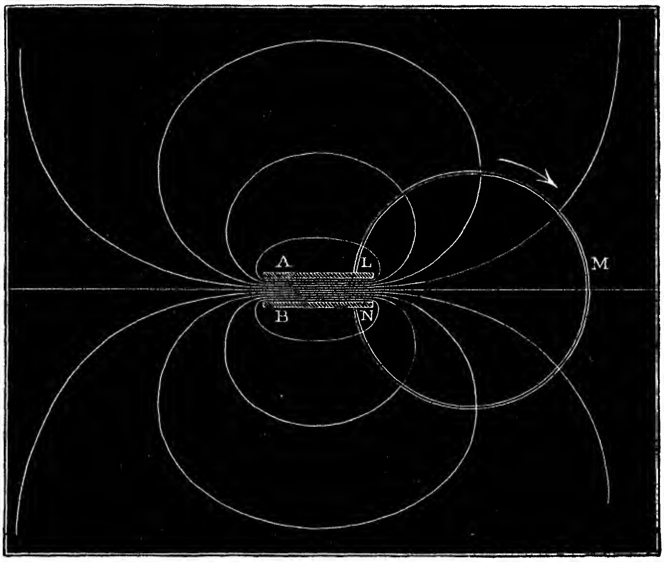
$$\frac{1}{4\pi}\iint [l(\mathrm{R}\beta-\mathrm{Q}\gamma)+m(\mathrm{P}\gamma-\mathrm{R}\alpha)+n(\mathrm{Q}\alpha-\mathrm{P}\beta)]\,d\mathrm{S},$$

where $d\mathrm{S}$ is an element of the closed surface, l, m, n the direction cosines of the normal to the surface, P, Q, R the components of the electromotive intensity, and α, β, γ those of the magnetic force. This expression may be regarded as showing that the energy flows across the surface, the components of flux being

$$\frac{1}{4\pi}(R\beta-Q\gamma),\ \frac{1}{4\pi}(P\gamma-R\alpha),\ \frac{1}{4\pi}(Q\alpha-P\beta);$$

the vector which has those components is now universally known as Poynting's vector; it is at right angles to both the electric and magnetic forces and is proportional to the product of these forces and the sine of the angle between them. Thus when we can draw equipotential surfaces for both the electric and magnetic forces the energy flows along the lines of intersection of the two sets of surfaces. Poynting illustrates this theorem by applying it to the following cases: a constant current flowing along a straight wire, a condenser discharged by short-circuiting the plates by a wire of great resistance, a voltaic battery, a thermo-electric circuit.

The magnitude of the change in the point of view consequent on the principles brought forward in this paper is perhaps shown most clearly in the case of the discharge of the condenser and the transference of the energy which before the discharge was distributed between its plates into heat in the discharging circuit. Before the publication of this paper the general opinion was the energy was transferred along the wire much in the same way as hydraulic power is carried through a pipe. On Poynting's view the energy flows out from the space between the plates and then converges sideways into the wire, where it is converted into heat, the paths of the energy being those represented in the figure.



As shown in this figure the paths of energy near the wire are at right angles to it. This is not so unless the wire is such a bad conductor that the lines of electric force in its neighbourhood run parallel to it; if for example the current through the wire were an alternating one with very high frequency the electric force near the wire would be at right angles to it. In this case the energy would flow parallel to the wire but outside it.

In the second paper Poynting, taking the view that the electromagnetic field consists of distributions of lines of electric and magnetic force, discusses the question of the transfer of energy from the point of view of the movement of these lines. He applies the same considerations to the question of the residual charge in Leyden jars in his fascinating and instructive paper on Discharge of Electricity in an Imperfect Insulator ('Phil. Mag.,' vol. 5, 1886, p. 419). Poynting's vector occurs as a quantity of fundamental importance in many theories of electromagnetic action in which the subject is approached from a point of view somewhat different from the one he adopted. It appears, for example, as a measure of the momentum per unit volume when the electromagnetic field is regarded as a mechanical system and the properties of the field as the result of the laws of motion of such a system. It appears, too, when we regard magnetic force as the result of the motion of tubes of electric force, the direction of motion of these tubes being parallel to Poynting's vector.

Pressure of Light.

For some years before his death Poynting devoted much attention to the question of radiation and the pressure of light. On the theory of this subject he published ('Phil. Trans.,' A, vol. 202) a very valuable paper, in the first part of which he discusses the application of the fourth-power law of radiation to determine the temperature of planets (in this he found afterwards he had been anticipated by Christiansen). Among other interesting results he arrived at the conclusion that the temperature of Mars must be so low that life, as we know it, would be impossible on its surface, this result was criticised by Lowell, but Poynting maintained his ground in a paper published in the 'Philosophical Magazine,' December, 1907. The second part of the paper in the 'Philosophical Transactions' contains investigations of the repulsive force between two hot spheres which arises from the radiation from the one tending to repel the other. He showed that if the bodies are in radiation equilibrium with the Sun at the distance of the Earth from it, the repulsive effect will be greater than the gravitational attraction between them if their radii are less than 19·6 cm., if their density were that of water; if they were made of lead the corresponding radius would be 1·78 cm. Thus if Saturn's rings consisted of very small particles it is possible that the effect of radiation might make them repel instead of attract each other. He considers at the end of the paper the effect produced by radiation on the orbits of small bodies round the Sun and shows that this would ultimately cause them to fall into that body. To quote his own words: "The Sun cannot tolerate dust. With the pressure of his light he drives the finest particles altogether away from his system. With his heat he warms the larger particles. They give out this heat again and with it some of that energy which enables them to withstand his attraction. Slowly he draws them to himself and at last they unite with him and end their separate existence." (' Pressure of Light,' 'Romance of Science ' Series.)

He made important contributions to the experimental side of the subject, thus with Dr. Barlow he established the existence of the tangential force produced when light is reflected from a surface at which there is some absorption, and also the existence of a torque when light passes through a prism. They also succeeded in demonstrating the existence of the recoil from light of a surface giving out radiation: an account of these experiments was given in the Bakerian Lecture for 1910 ('Proc. Roy. Soc.,' A, vol. 83, p. 534, 1910). These investigations involved the detection of exceeding minute forces and gave ample scope for Poynting's skill in devising methods and apparatus. He had exceptionally good mechanical instincts and an excellent knowledge of the capabilities of instruments; the result was that the apparatus he designed was always simple and effective.

In addition to papers published in scientific journals and the Transactions of Society he wrote 'The Mean Density of the Earth: The Adams Prize Essay for 1893,' 'The Pressure of Light' ('Romance of Science' Series) and 'The Earth' (Cambridge University Press). Of the 'Text Book of Physics' written in conjunction with J. J. Thomson he wrote the whole of the volume on Sound and Heat and of the first volume of Electricity and Magnetism and the chapters on Gravitation in the Properties of Matter. His writings exhibit to the full the clearness, simplicity and thoroughness which was characteristic of all his work.

J. J. T.

JAMES FRANCIS TENNANT (1829–1915).

LIEUT.-GENERAL JAMES F. TENNANT, C.I.E., R.E., was born at Calcutta January 10, 1829, being the eldest son of Captain (afterwards Brigadier-General) James Tennant, of the Bengal Artillery, who commanded the Artillery during the last Sikh War and received the K.C.B. for his services. The son entered the service of the East India Company, after the usual training, in June, 1847; a beautiful little sextant, which he used throughout his life to excellent purpose, bears the inscription: "Presented at the Public Examination on June 11, 1847, to Gentleman Cadet James F. Tennant by the Honourable Court of Directors of the East India Company as a mark of the Court's approbation of his attainments in Mathematics while at the Military Seminary." He landed at Calcutta as 2nd Lieutenant in the Bengal Engineers in March, 1849, was promoted 1st Lieutenant in 1854, 2nd Captain in 1858, 1st Captain 1862, and so through other promotions to full Colonel on the last day of 1878.

He seems to have been attached from the time of his landing in India to the Great Trigonometrical Survey. The magnificent reports of this Survey have apparently not yet been indexed, so that it is not easy to trace the history of individuals, though full details of *personnel* are given under the heading of each operation. In vol. 2, p. 12, we read that the Karachi longitudinal series was completed in four field seasons, ending in April, 1853, by Captain A. Strange, "with occasional assistance from Mr. C. Lane and Lieutenant J. T. (*sic!*) Tennant." A later reference shows that the T. is a misprint for F., and since the interval of four seasons carries the date back to 1849, the year of Tennant's landing in India, it may be presumed that he began this work at once. He was then put in charge of a part of the Great Indus series, starting at the Karachi base, and carrying the survey 90 miles to the north in 1853–4, adding another 96 miles in 1855–6. He was then transferred to the Jogi Tila series (meridian 73½°) and did 60 miles, before the outbreak of the Mutiny led him to volunteer for active service in August, 1857.* He was appointed to the Delhi Field Force as Field Engineer, and afterwards Garrison Engineer, was transferred to Lucknow in 1858, and was attached to that force during the siege of Lucknow, being mentioned in General Outram's roll of officers deserving honourable mention at the siege.

There is a letter from him in 'Mon. Not. R.A.S.,' vol. 18, p. 287 (June, 1858), relative to the instruments of the Lucknow Observatory, which opens thus:—

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* The writer of this notice has been privileged to see the statement of service drawn up by Colonel Tennant in 1882. It is remarkable, as illustrating tricks of memory, that these important dates are there quoted one year wrong, putting the Mutiny in 1858-59!

"I believe I was the only member of the Society present at the capture and occupation of Lucknow, and I think possibly some account of what has been the fate of the observatory and instruments may, though removing almost all hope of the latter ever again being found, be not uninteresting."

The following paragraph from vol. 2, p. 17, of the 'Great Trigonometrical Survey,' referring to these dark times, is of peculiar interest to us at the present moment.

"Though in this period the operations had to be suspended for some years, in several quarters, in consequence of the troubles caused by the Mutiny of the Bengal Army, yet—while all India was distracted by war and tumult—these works of science and peace advanced steadily, though at a slower rate of progress than would otherwise have been possible; the surveyors were transferred from districts where they would certainly have been murdered, to others where they probably would not be, and, as it happily turned out were not, murdered. Several of the Military Officers quitted the survey temporarily, and served at the siege of Delhi, the relief of Lucknow, and in various actions against the rebels; others had the more trying duty to perform of remaining to continue the work of the survey, when they would rather have joined their comrades, in the struggle on which the fate of the British Empire in India was depending. When it was nearly all over, Lord Canning, the Governor-General of India, wrote a letter to Colonel Waugh, acknowledging his reports of the operations of this Survey, from which the following paragraph has been extracted:—

"'I cannot resist telling you, at once, with how much satisfaction I have seen these papers. It is a pleasure to turn from the troubles and anxieties with which India is still beset, and to find that a gigantic work, of permanent peaceful usefulness, and one which will assuredly take the highest rank as a work of scientific labour and skill, has been steadily and rapidly progressing, through all the turmoil of the last two years.'"

In the latter part of 1858 "operations were commenced on the [Gurhagarh Series, Meridian 75°] at the north end during 1858–59, by the Rahún Party, under Major Tennant, the country through which the latter series lay being too disturbed from the effects of the mutiny to admit of its being continued without risk to the safety of the instruments, and the members of the survey employed" (vol. 4, p. 7, F). But the breakdown in health of Captain Jacob, Director of the Madras Observatory, led to the appointment of Major Tennant as temporary Director (October, 1859–October, 1860) pending the selection of Mr. N. R. Pogson, who reached India early in 1861. Tennant apparently did not return to survey work, and the noteworthy incidents in the remainder of his career centre in the observations of the total eclipses of 1868 and 1871, and the transit of Venus in 1874. He officiated as Master of the Mint in Calcutta on various occasions, and was ultimately in permanent charge from January 18, 1876, to February 6, 1882, when he retired and came to England. He became a constant attendant at the meetings of the Royal Astronomical Society, served on the Council

1885–1894, and was President in 1890–91. He was elected F.R S. in 1869 after the successful observations of the eclipse of 1868.

Apart from the eclipse work, Tennant's astronomical papers call for little notice at this day. His first paper, presented to the Royal Astronomical Society soon after his election to its Fellowship in 1855, dealt with a method "of relieving the weight of the moving portion of an altazimuth," and is characteristic both of the man and the times. His suggestion was to obtain "hydraulic relief" by means of an oil reservoir. He was an alert observer, reluctant to accept an instrument as finally satisfactory; and though nothing seems to have come of his suggestion at the time, he had the pleasure, many years later, of seeing his friend, Dr. A. A. Common, use "hydraulic relief" successfully in the mounting of his huge five-foot equatorial. In subsequent papers Tennant considered improvements for the sextant, the transit, the chronometer, the prime vertical transit, the pendulum, and even the tables of logarithms necessary for use with all of them. He was ungrudging of labour, and spent the leisure of his retirement in England in compiling a useful table of the positions of 400 or 500 observatories with the parallax factor for each; or computing the orbits of various comets; or tracing the formation of telescopic images by numerical methods. It would serve no good purpose to follow these activities in detail here, for the sympathetic attention they once obtained is passing away, if it is not already gone, with those who solved the problems pressing for solution half a century ago. A single instance will suffice to remind us of the change that has come over astronomical work. In one of his papers, as late as 1875, Tennant calls attention to three stars within 10° of the Pole, which Groombridge had noted as of 6 magnitude. Apparently more stars near the Pole were wanted for survey work, and it was disappointing to find that these three were not to be seen in the instruments used. Were they, perhaps, variable? Tennant's only resource was to call attention to the matter in the hope that further observations might throw light on it. Nowadays it is perfectly easy for us to turn to half a dozen catalogues, all of which show that Groombridge's estimates are two magnitudes in error, while as regards variability (though the evidence can, of course, never be complete) the presumption is strongly against any sensible change. We have almost forgotten how little was known about casual stars half a century ago.

There is, however, another side to Tennant's work which reveals more of his qualities. His alertness made him a pioneer in new regions that are still being explored. The eclipse of 1868 marked an epoch in the history of solar work: it has become famous by giving Janssen* the clue which enabled him to see the chromosphere in full daylight. It was Tennant who first called attention to the favourable nature of this opportunity nearly two years beforehand, in a paper to the Royal Astronomical Society. He calculated the requisite data: he was entrusted by the Government, on the recommendation of the Royal Astronomical Society, with the general arrangements for observation, and he carried them out very successfully. Moreover, he showed the true spirit of enterprise by his anxiety to use the photographic method, then comparatively new: and here, again, it is hard for us to realise what it meant to override strong traditions (even some prejudices), and to face the difficulties of wet-plate photography. He did not make the actual exposures himself, though that had been his first intention when he had the promised assistance of Lieutenant (now Colonel) Herschel to take charge of the spectroscope: on the latter being detached for duty elsewhere, Tennant reluctantly took charge of the spectroscope himself, but he instructed the photographers in the necessary operations, initiating that system of drill which has become a regular feature of eclipse work. [The novelty of such work at the time is illustrated by a footnote in Tennant's report commenting on the desirability of a good supply of tools !] The photographs showed only the merest traces of Corona, a failure which, from subsequent experience in 1871, Tennant ascribed to the haze obscuring the sun. But this did not prevent spectroscopic observations by Tennant (who found the spectrum continuous), and polariscopic by Captain Branfill, (who found strong polarisation), and the former accordingly summed up his conclusions as follows :—

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* Sir Norman Lockyer independently made the same discovery, and though he only observed the chromosphere lines after the eclipse results became known in this country, the instrument by means of which they were observed was in the hands of the maker before the eclipse took place, and there is no reasonable doubt that he would have seen the lines even though they had not been observed during the eclipse.

First :—The Corona is the atmosphere of the Sun, not self-luminous but shining by reflected light.

Secondly: The Great Horn (prominence) certainly was composed of incandescent vapours, and probably all the brilliant protuberances are the same.

We have so little quarrel with these statements to-day that it is almost difficult to read them with interest. But the environment of the time was strangely different. Many astronomers still thought that the corona was a terrestrial, or perhaps a lunar phenomenon, a view which was strengthened by the observations made in America at the 1869 eclipse. For one thing, the Americans found its light unpolarised. This will suffice to show that Tennant's observations and conclusions were at least a useful contribution on the right side at a critical time.

In view of the "contradictions which the American eclipse of 1869 had produced, and to reconcile these, if possible," Tennant observed the eclipse of December 1871, this time with Captain Herschel's help, as also that of Mr. J. B. N. Hennessey and Captain J. Waterhouse, to whom the photographs were entrusted, with a success which has become famous. It was recognised that the light of the corona was not wholly reflected; but we may fairly ascribe Tennant's failure to see the bright lines in 1868 to the same haze which obliterated the corona from the photographs. The conditions in 1871 were clearly much better.

The transit of Venus in 1874 was a piece of thoroughly congenial work for Tennant. After this his duties took a more business-like form until his return to England. When the Astrographic Conference of 1887 was called, he was very naturally one of the British delegates. As President of the Royal Astronomical Society he delivered an address on the work of Sir G. H. Darwin, there being no medallist in his first year of office. As already intimated, he was keenly interested in the large reflecting telescopes set up by Dr. Common at Ealing, and always ready to work out any special mathematical problem which arose in the course of the work.

For the last 20 years he has not attended scientific meetings. He died on March 6, 1915, at the ripe age of 86.

H. H. T.

WILLIAM JAMES SELL, 1847–1915.

William James Sell was born in 1847 at Cambridge, and had his early education at one of the primary schools of his native town. There his personal character and rapid progress in learning soon attracted the notice of the master. His scientific education began at the chemical laboratory of St. John's College, where, at the age of 14, he was employed by the Professor of Chemistry. At that time this laboratory, maintained by the College and, with great liberality, placed at the disposal of the University Professor, was the only place in Cambridge where undergraduates could get any instruction in practical chemistry. Subsequently in 1866, when the University provided some rooms for a students' laboratory, Sell became lecture assistant to the Professor of Chemistry. He was one of those who knew how to take pains and soon made himself proficient in his duties, and became the professor's right hand in his efforts to establish a school of chemistry. Those were days of small things, when the University had little money, and when the prejudice against physical science may be gathered from the words of an eminent literary professor, who said that the University was no more called on to teach practical chemistry than to teach shoe-making. Sell's heart was in his scientific work, and the students, mostly beginners, acknowledged the help he freely gave them in the difficulties of manipulation. He had married in 1870, and, as soon as circumstances permitted, he matriculated in the University as a member of Christ's College, and with characteristic courage set to work, in vacation and in the evenings when the laboratory was closed, to learn Latin and Greek, of which, heretofore, he knew nothing. He passed all the preliminary examinations without one failure, and in 1876 obtained first class honours in Natural Science at the final examination for the B.A. degree. Having this proof of his qualification for teaching, he now changed his position in the laboratory and took up the post of Demonstrator, and on the retirement of Dr. Hicks (afterwards Bishop of Bloemfontein), succeeded him as principal Demonstrator, and retained that post to the end of his life.

To the way in which he fulfilled the duties of his position the laboratory owes much of its prosperity, its rapid expansion, and the after success of those trained in it. Never making difficulties himself, he nevertheless did not belittle the difficulties of others, and was most willing and patient in his endeavours to remove them. His love of doing everything well, and his unwillingness to take for granted anything of which the truth could be tested experimentally, were reflected in the work which he superintended, and had a prevailing influence on his pupils. His teaching, however, was not long confined to demonstrating, for he was always ready, wherever a gap appeared, to try and fill it to the best of his ability. Such gaps were frequent in the early days when the resources of the University were unequal to meet the ever-recurring demands made by the rapid development of physical science and Sell came to be really an efficient assistant professor, taking a full share of the lectures and of all the instruction in chemistry. The University never properly recognised what he did. He ought to have been given the office of Reader in Chemistry. Sell himself was so modest, and so free from any kind of self-assertion, that his merits and his work were little known outside the laboratory, and when a movement was initiated to get him appointed Reader, it was not denied that he had a good claim, none a better, but the authorities seem to have thought, because readerships were demanded in other departments and the University had not the means of endowing them all, that the easiest course was to refuse them all. Sell never made any complaint of being unappreciated. All that was done for him was to give him a University Lectureship with a stipend of £50 a year, and he frankly accepted it.

Notwithstanding his close attention to official duties, he found some time for original researches in more than one line. Of these, the most important were a long series of investigations on pyridine derivatives, which were the subject of more than a score of papers published in the 'Transactions of the Chemical Society of London.' In some cases he was assisted in this work by one or other of the Junior Demonstrators, and their names appear as joint authors of the published papers. In others he was sole author, and the thoroughness of the work is characteristic of him. Many new products were obtained and their properties carefully observed. Of these there are hardly any of which he failed to establish satisfactorily the chemical constitution; and the whole form a substantial addition to Chemical Science. Interesting communications by him on the salts of a base containing chromium and urea were published in the 'Proceedings of the Royal Society,' and on colloid solutions of ferric and other phosphates in the 'Proceedings of the Cambridge Philosophical Society.'

In 1900 he was elected a Fellow of the Royal Society of London, and in 1906 he took the degree of Doctor in Science in Cambridge.

His personal character is pretty well indicated in the foregoing remarks. His colleagues all testify to his loyalty in the cause of the advancement of learning, and to the unfailing, but unobtrusive, way in which he assisted them whenever occasion called for it; and many of his pupils own, not merely how much they learnt from him, but the affection with which his personal influence inspired them.

G. D. L.

ARTHUR AUWERS, 1838—1915.

THE life work of Auwers consisted largely in the co-ordination of astronomical observations. Since the time of Bradley, 1755, and particularly in the nineteenth century, the positions of many thousands of stars have been determined at many observatories. Not only are the observations affected by accidental errors of very different amounts, but they are also liable to systematic errors. The determination of these systematic errors and the value to be attached to different observations is of more difficulty and importance than would appear at first sight. The reason for this is that the stars are all in motion *inter se*, and that the proper motions or angular movements of the stars are of as great importance as their relative positions at a given epoch. As these angular movements are comparatively small, a considerable increase in the accuracy with which they may be determined is secured by careful discussion and refinement of the observations. Particularly is this necessary for determinations of precession, solar motion or systematic movements among the stars. Three astronomers, Auwers, Newcomb, and Boss, rendered pre-eminent service in this work during the last 50 years. Thanks to their efforts the positions and movements of the brighter stars have been determined with great accuracy, by making use of the best observations and those which are most free from systematic error, spread over as wide an epoch as possible. The positions of these stars have then been used to derive the systematic errors of many star catalogues, which have thus become available for the more accurate determination of the positions and movements of many thousands of fainter stars.

George Friedrich Julius Arthur Auwers was born at Göttingen on September 12, 1838. His astronomical career began at the observatory of that University in 1856. As a student of astronomy he made meridian observations of comets and minor planets, varying his occupation by an occasional computation of orbits. In addition he was interested in variable stars, whose importance had been brought prominently forward by Argelander. Auwers made observations of many of the variables then known, including the famous stars ο Ceti and β Lyræ, and discovered the variability of several other stars, among others of δ Orionis and ο Tauri. It was also his fortune a few years later to discover a new star, "Nova Scorpii."

In 1859 he was appointed assistant at the Observatory of Königsberg, the scene of Bessel's labours from 1810 to 1846. He remained at Königsberg only three years, but in this short time determined the parallax of Lal 21258, ζ Ursæ Majoris, Procyon and 61 Cygni with the famous heliometer with which Bessel had determined the parallax of 61 Cygni in 1838. He also made an exhaustive investigation of the irregularities in the proper motions of Sirius and Procyon, to which Bessel had drawn attention and attributed to the presence of massive but invisible companions. These companions were discovered many years later, and the periods and other elements of their orbits determined by Auwers were closely verified.

From 1862 to 1866 Auwers was an assistant at the Observatory at Gotha. Here he added to his determinations of stellar parallax by finding that of Groomb 34 by observations of transits with an equatorial at the two periods of parallactic elongation. The result was very satisfactory and agrees closely with modern photographic determinations. While at Gotha he published in 1865 his first important contribution to fundamental astronomy. This consisted in the establishment of a "Fundamental System of Declinations" and a determination of the systematic corrections required by different catalogues to bring them into harmony with it. The basis of this system of declinations is the mean of 13 catalogues dating from 1820 to 1860, including those of Bessel, Struve, Argelander, Pond, Henderson, Johnson (St. Helena), and Airy. The proper motions were obtained by comparison with Bradley's positions for 1755 as given in Bessel's 'Fundamenta Astronomiæ.'

In 1866 Auwers was appointed a member of the Academy of Sciences and changed his residence from Gotha to Berlin. He commenced the great work of the re-reduction of Bradley's observations about this time and pursued it assiduously for 10 years. Bradley's observations were made at Greenwich from 1752 to 1760, with a new transit instrument and quadrant, both executed with great care and skill by Bird. After Bradley's death the observations passed into the hands of his executors, and were finally presented to the University of Oxford. Forty-three years after Bradley's death they were published by the University in two folio volumes, the first of which, edited by Hornsby, appeared in 1798, and the second, edited by Robertson, in 1805. The observations as published were unreduced. In 1807 Olbers presented his copy to Bessel, who undertook their reduction and published the results in 1819 in his 'Fundamenta Astronomiæ.' The progress of astronomy in the first half of the nineteenth century, to which this work largely contributed, made a further and more complete reduction of Bradley's observations both possible and desirable. Auwers made the re-reduction with characteristic thoroughness. He obtained Bradley's manuscripts and went through them figure by figure. The mean position for 1755·0 was calculated for each observation, and numerous errors were detected by the comparison of the different observations of the same star. The instrumental errors were thoroughly investigated, and particular care was devoted to the relative position of the clock-stars so that the intervals might be correct for stars distant from one another in right ascension. Similar care was lavished on the determination of the index errors of the quadrant. For this purpose Auwers reduced 1427 observations of 85 stars made by Bradley with the zenith sector. He supplemented these by a series of observations made by Maskelyne between 1768 and 1786, and a series made by Bradley at Wanstead. In this manner he compiled a catalogue of the zenith distances of 130 stars distributed over the 24 hours of right ascension, which served to give zero points for all the quadrant observations.

This re-reduction gave accurate positions for the epoch 1755 of 3268 of the brightest stars observable in the latitude of Greenwich. Re-observation of the stars at Greenwich gave their positions for the epoch 1865, and a comparison of the observations gave proper motions with great accuracy. The whole work was published in three volumes by the Petrograd Academy of Sciences. Vol. 3, giving the catalogue for 1755 and the proper motions, appeared in 1888; vol. 2, giving the ledgers of the separate observations, in 1882, and vol. 1, giving the details of the reduction, in 1903. The Gold Medal of the Royal Astronomical Society was awarded to Auwers in 1888. The President of the Society, Dr. Glaisher, expressed in his address the judgment of astronomers on this great work as "admiration of the manner in which the most refined skill has been combined with the most patient care in its performance" ('M.N., R.A.S.,' vol. 47, pp. 236–251).

Soon after its formation in 1865 the Astronomische Gesellschaft decided to form a catalogue of all stars down to the ninth magnitude between the limits of −2° and +81° dec. It was considered that this great project would be carried out most efficiently and economically by making the observations differential. The construction of the fundamental catalogue on which the observations should depend was entrusted to Auwers. He took the Pulkowa catalogue of 1865 as a standard with proper motions obtained from comparison with Bradley. With this provisional system he compared other catalogues and deduced systematic corrections to them. Applying these corrections to the various catalogues, he then derived new positions and proper motions for each star from all the observations, thus obtaining the greatest accuracy for each star, but making the fundamental catalogue systematically that of Pulkowa 1865 and Bradley 1755. This catalogue was published in 1878, and was the system of star places adopted by the 'Berliner Jahrbuch.' Later it was extended from dec. −2° to dec. −23° to serve for the Southern zones subsequently added to the programme of the Astronomische Gesellschaft.

The system of declinations of Auwers' catalogue was strongly criticised by Boss on the ground of errors of unknown amount in the flexure of Bradley's quadrant, which introduced error of systematic character in the proper motions. Boss, in forming his system, elected to use no observations earlier than Bessel's of 1820. The proper motions obtained by comparison of the excellent observations of Bessel, Struve, and Argelander from 1820 to 1830 with those taken near the end of the nineteenth century proved the accuracy of Boss' contention. But though the observations of Bradley can no longer be taken as the origin of a fundamental system, the large number of stars, and their comparatively small accidental error, make Auwers' Bradley, when corrected for the systematic errors due to the flexure of the quadrants, the most valuable source for the proper motions of stars. It has formed the basis for determinations of the constant of precession, the direction of the solar motion and other cosmical researches, and was the material used by Kapteyn in his discovery of two star streams.

Besides the formation of the fundamental catalogue, Auwers contributed to the great catalogue of the Astronomische Gesellschaft the zone from +14° 50′ to +20° 20′. He himself observed the transits of 9789 stars, each star being observed at least twice, and the circle readings being generally made by Dr. Romberg. The bulk of the observing was done between June 29, 1869, and August 12, 1871, a few observations being made later to clear up errors and fill gaps caused by bad weather. The instrument employed was the old meridian circle of the Berlin Observatory, constructed by Pistor in 1829 and redivided by Martin in 1845. The observations do not reach the high accuracy obtained with the new transit circle of the Berlin Observatory, with which the zones 20° to 25° and 70° to 75° were observed. The catalogue is, however, remarkable for the fullness and thoroughness of the comparisons with earlier catalogues, which have made it a most valuable source of information on the proper motions of faint stars.

After the re-reduction of Bradley's observations Auwers constantly had in mind the possibility of a better reduction of several old catalogues. The difficulties are very much reduced when once the accurate positions of a number of stars are known for the epoch required, as the positions of other stars may be derived differentially from these. Auwers himself reduced Mayer's observations made at Göttingen from 1756 to 1760. This catalogue, published in 1894, contains 1027 stars, of which about one-third are not found in Bradley. Only last year he also published a catalogue of 4219 stars for the epoch 1745 from the observations made by Bradley at Greenwich before Bird's new transit instrument and quadrant were installed. The utilisation of the old observations appealed strongly to Auwers, and by his advice the Berlin Academy embarked on a comprehensive scheme, the 'Geschichte des Fixsternhimmels." The purpose of this project was to collect all the meridian observations from 1750 to 1900, and to form therefrom one general catalogue for the epoch 1875. The proper motions are to be deduced whenever there is sufficient material. The total number of published star places obtained by meridian observations is approximately 1,000,000, relating to 250,000 stars. This work was begun in 1898 by Dr. Ristenpart, and has been carried on since 1900 by the Berlin Academy under the direction of Auwers.

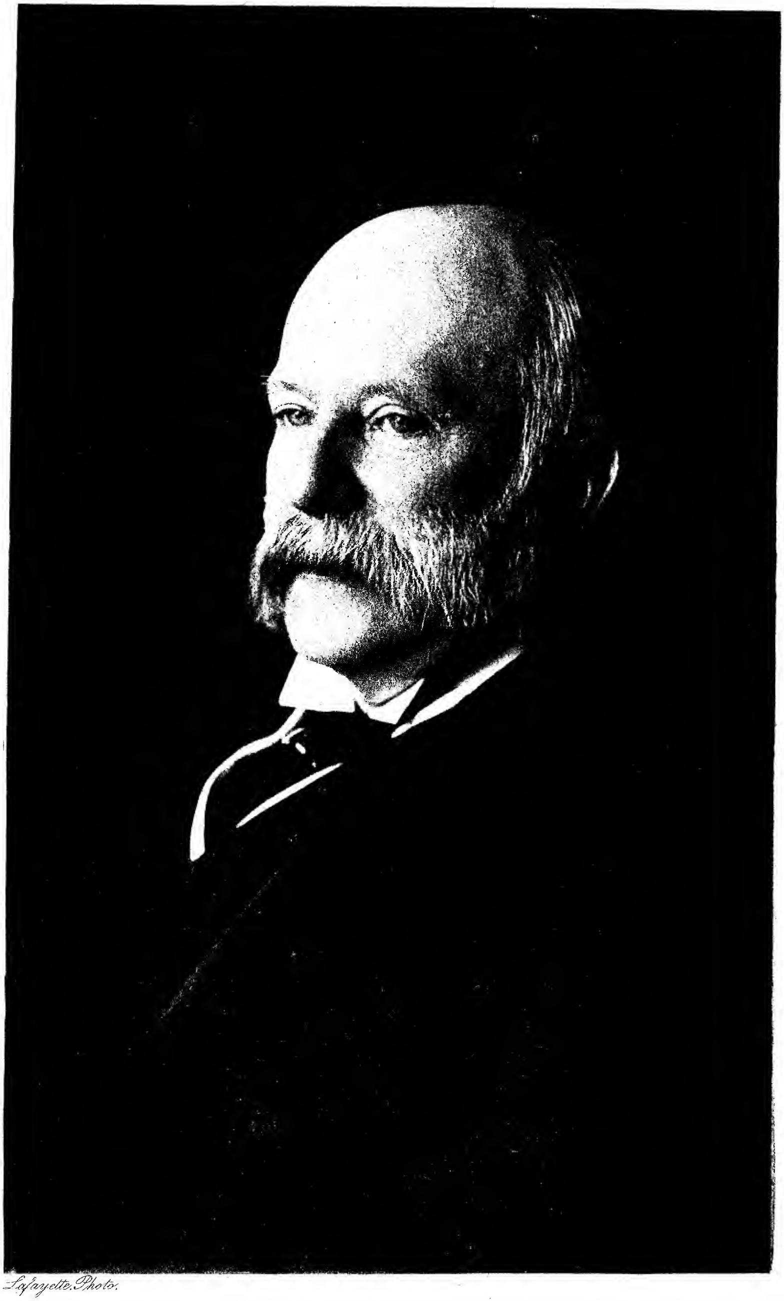
It fell to Auwers as a member of the Commission for the observations of the transit of Venus to take a large share in the organisation of the German expeditions. Only at the last moment was he able to go himself to observe the transit of 1874. On that occasion he went to Luxor to make observations of a subsidiary character on the form of the planet Venus. In 1882 he took a larger share in the observations, being in charge of the expedition to Punta Arenas in South America. The reports of the German expeditions in 1874 and in 1882 were drawn up by him and are published in six large volumes.

Another astronomical expedition made by him was to the Cape Observatory in 1889, when he assisted Gill in heliometer observations of Victoria for the determination of the solar parallax. At Gill's request he undertook the discussion of the meridian observations of the stars required in the heliometric triangulation. This investigation, carried out with his usual thoroughness, served to bring into prominence the variation of personal equation in observing stellar transits depending on the brightness of the star.

Auwers was widely known and honoured. Since 1878 he had been Secretary of the Academy of Sciences of Berlin. From its foundation in 1865 to 1874 he was Secretary of the Astronomische Gesellschaft, and from 1881 to 1889 its President. He received many marks of distinction in recognition of his work, including the Foreign Membership of the Royal Society in 1879 and the Gold Medal of the Royal Astronomical Society in 1888 from Great Britain. Only two years ago the fiftieth anniversary of his doctor's degree was celebrated by the presentation to him of a copy of the portrait of Bradley in the possession of the Royal Society, and by the institution of the Bradley Medal of the Royal Prussian Academy of Sciences. This medal bears the inscription, "In memoriam J. Bradley et F. W. Bessel astronomorum illustrium," and round the edge, "Condidit A. Auwers die XXV Junii MCMXII." Auwers was a prodigious worker, who thought no labour too arduous if only greater accuracy could be secured. Astronomy is pre-eminently a study in which this is necessary, and the labours of such as Auwers are indispensable. His name will be associated with those of Bradley and Bessel, whose brilliant work was made of greater value by his labours.

He died on January 24, 1915.

F. W. D.



Lafayette Photo.

Arthur W. Rücker

Sir ARTHUR RÜCKER (1848-1915).

Arthur William Rücker was born at Clapham on October 23, 1848. His father, Daniel Henry Rücker, was the senior partner of the firm of J. A. Rücker and Bencraft, Colonial Merchants, of Mincing Lane, originally established in London by Johann Anton Rücker about the middle of the eighteenth century. The family can trace its lineage back to the Middle Ages. The records of Hamburg show that a branch was settled in that city in 1700 and that between 1767 and 1869 five of its members served on the Senate.

In the formation of his character Arthur Rücker owed much to the example and precepts of his parents. As a boy he was of somewhat delicate health. He had inflammation of the lungs when very young and was supposed by the family doctor to have had a weak chest. He had a tendency to headaches, due probably to some latent defect of eyesight which was corrected in later life. He was highly sensitive and of a shy retiring disposition, fond of reading and keenly interested in his lessons, but with little aptitude for the rougher sports of boyhood. Not that he was disinclined towards them: it was rather that he was restrained from indulging in them on the ground that his physical strength was not equal to violent exercise.

For this reason it was thought undesirable to send him to a public school away from home, and accordingly, after a few years at a preparatory school in the neighbourhood, he was entered as a day boy at the Clapham Grammar School, then under the direction of the Rev. Charles Pritchard, F.R.S., afterwards Savilian Professor of Astronomy at Oxford. The school had a considerable reputation at the time, and was selected by Charles Darwin for his four younger sons, partly because of its nearness to Down, but mainly for the reason that the education was not so strictly classical as in the larger public schools. Mathematics formed a considerable part of its work. It had in it the beginnings of a "modern side," and occasional lectures on chemistry, heat, and electricity were given.

Dr. Pritchard resigned the Principalship in 1862, whilst Rücker was still a junior boy, and consequently most of his higher school work was done under the direction of Dr. Wrigley, who had succeeded to the headmastership.

The school authorities were of opinion that Rücker's strongest side was literature and classical languages, and accordingly when he reached the head of the school he was entered for a classical scholarship at Corpus Christi, Oxford. There was some idea in home circles at this time that he might eventually go to the Bar. In a letter to his father describing the course of the examination for this scholarship, and his impressions concerning it, after dealing with the classical part, about which he seemed a little dubious, he turns to the mathematics: "I did the whole paper except one question, and one question I answered both by Geometry and Conic Sections, another both by

Trigonometry and the Differential Calculus. I had determined if possible to show them I knew a little of the last. . . . Very few took the mathematical paper, and some of the fellows when they saw me doing so much stared at me as if I were a newly imported wild beast . . . . I only hope it was all right. S. seems to think I have a chance, but I think now, as I always did, that from the classical nature of the College it is very uncertain."

Rücker's prevision was correct. He was not successful in securing the Corpus Christi Scholarship, but his mathematical work so impressed Mr. Esson, one of the examiners, who was reported to have said that if he had gone in at Merton he would have given him their Mathematical Postmastership, that he was strongly advised to devote himself to that subject. Accordingly in the following year (1867) he tried for an open mathematical scholarship at Brasenose and was successful.

Rücker, as may be supposed, took his university life seriously, and his home letters constantly bear witness to his sense of duty and responsibility. He was ambitious to make a career; and success at Oxford, as he fully recognised, was a sure avenue to it. Of a frank, genial disposition, he quickly made friends, choosing them with discrimination from among steady working men of habits and tastes similar to his own. He joined the Brasenose Company in the University Rifle Corps, "as a matter of College honour," and was a regular attendant at drills and shooting competitions, and as a sergeant shared in parades and Easter Monday reviews. He occasionally went on the river and found to his great delight that he could pull an oar as well as the average man in a scratch four or eight. He took a loyal interest in the athletic successes of his college, played fives and skated when he had the chance. But his favourite form of recreation was walking, and a country ramble was a constant delight, and no one ever learned to know the country-side of Oxford better.

In 1869 Rücker gained the Junior University Mathematical Scholarship, a First Class both in the First and Second Public Examinations for Honours in Mathematics, a First Class in the Honour School of Natural Science, and in 1871 an open Mathematical Fellowship at Brasenose, after his thirteenth term of residence—a hitherto unprecedented occurrence—when he was appointed Mathematical Lecturer of his College, and demonstrator, under Prof. Clifton, at the Clarendon Laboratory. During his tenure of this post he gave a course of lectures in Physics to an audience of one, the late Duke of Albany. He resigned his fellowship on his marriage in 1876, but was elected in 1891 as one of the first three Honorary Fellows when Brasenose acquired that privilege.

Rücker published his first scientific paper whilst at Oxford, a short communication to the 'Phil. Mag.' of 1874, "On the Adiabatics and Isothermals of Water,"—a mathematical criticism of a theorem in Verdet's 'Thermodynamics.' It might be thought that to a man of Rücker's tastes and temperament there was much in Oxford to incline him to cast in his lot with the University. But forty years ago its attitude towards physical science afforded little encouragement or prospect of a career to one of his energy, capacity and high sense of duty and public usefulness. Accordingly, when the Yorkshire College of Science was founded in 1874, he offered himself as a candidate for the Chair of Mathematics and Physics.

Among his testimonials was one from the Principal of Brasenose, who testified that, in addition to his academic qualifications, he was "possessed of a cheerful temperament and of remarkably pleasant manners, with other external advantages not unimportant to the teacher's office . . . . I should be sorry to lose him, but I am quite aware that Oxford at present offers few openings to an ambitious votary of natural science."

Oxford's loss was Leeds' gain, and the "ambitious votary" was elected. He had as colleagues Prof. Thorpe and the late Prof. Green, who afterwards held the Chair of Geology at Oxford. These were the first, and for a time the only, teachers in the institution. They began their work with very modest appliances and in temporary premises ill-fitted for the purpose of instruction in physical science. This, however, is not the place to dwell at length upon the origin and progress of the Yorkshire College, or to show how it has since grown into the University of Leeds. It is sufficient here to state that Rücker, from the beginning, clearly foresaw its latent possibilities, and strove to place it upon a more liberal basis than was contemplated by at least one section of its projectors. His quick intelligence, perspicacity, and soundness of judgment were at once appreciated by colleagues no less earnest in their endeavours to enlarge its restricted educational aims. He soon gained, too, the respect and confidence of the governing body. In spite of difficulties, due partly to its poverty and partly to the divergent policy and occasional vacillation of its Council, the institution, with a wider curriculum embracing not only physical science but also modern and classical languages, literature and history, was gradually brought to model itself upon the example of Owens College, with similar aims and aspirations.

Rücker also took a leading part in the deliberations which preceded the purchase of the site on which the present University buildings stand, and by a happy coincidence the foundation-stone of the first block was laid on the anniversary of his birthday—October 23, 1877—by the then Archbishop of York. He gave much time and thought to details in the planning of the new buildings, and personally inspected nearly every institution in the kingdom that might furnish suggestions as to the best form of internal arrangement.

In this year Owens College applied to the Privy Council for a grant of a charter of incorporation as the University of Manchester, with power to grant degrees in arts, sciences, medicine and law. As this action was bound to have considerable influence upon higher education in the North of England, and might seriously affect the future of the Yorkshire College, it was naturally viewed with some perturbation by those who had the interests of the young and struggling institution at heart. Accordingly, representations were made to the Privy Council in a memorial drafted by Rücker, which had the effect of delaying the grant of a charter in the hope that some modification of its scheme in the interests of the Yorkshire College might be found possible. Friendly communications were entered upon with the authorities of Owens College, with the result that eventually a joint memorial was presented, praying that Her Majesty Queen Victoria might create a new University in which the Owens College, and such other institutions as might be able to fulfil the conditions of incorporation, might be constituent colleges with a definite share in the government of the University. Such, in brief, were the steps immediately preceding the establishment of the Victoria University. In the delicate negotiations needed to reach this solution Rücker took a leading share, and it was in no small degree due to his tact, urbanity and constructive ability that it was secured.

In addition to his regular courses Rücker, in common with his colleagues, did a considerable amount of "missionary work" in the service of the College, lecturing on scientific subjects at various times, in practically every important town in the West Riding. One of these courses, given under the auspices of the Gilchrist Educational Trust, was subsequently published in volume form and had a certain measure of success in its appeal to a wider public.*

His remarkable gift of lucid exposition, which made him an admirable teacher, rendered his lectures, on which he spared no pains, interesting and attractive, and he could always count upon a "full house." He soon acquired a leading position in the intellectual life of the town and became associated with many of the educational agencies of the district, and was always ready to do what he could to advance and elevate the ideals of culture in the community. He was a member of the Leeds Educational Council and for seven years Hon. Secretary of the Leeds Philosophical and Literary Society. He had married, in 1876, the second daughter of Dr. Heaton, the Chairman of the College Council, an influential physician in Leeds, a man of great public spirit, and deservedly respected, and this union, combined with his many social gifts, naturally tended to widen the circle of his acquaintance.

For two years he was Chairman of the Academic Board and representative of his colleagues on the Council of the College. When it was eventually determined to appoint a permanent Principal the post was, in the first instance, formally offered to him, but declined on the ground that with the prospect of better accommodation in the new buildings then in course of erection, the arrangements of which, as regards the physical laboratories, were being carried out in accordance with his designs, he desired to confine himself to scientific work without the interruption of administrative duties. Up to this time he had been able to do comparatively little in the way of experimental inquiry. In the temporary buildings in which the College was first housed, no provision in the way of a physical laboratory was possible. Such practical work as he could set his pupils to do was restricted, not only by the lack of accommodation, but by the limited amount of his apparatus. Indeed, as the new laboratories approached completion, this latter circumstance, in view of the straitened finances of the College, occasioned him considerable anxiety, from which he was happily relieved by the generous and timely gift of £200 from his father towards its purchase. Accordingly his earliest work was largely confined to questions in which his mathematical powers could be utilised. His first contribution from the College was "On the Expansion of Sea-Water by Heat," a joint investigation, of which the observational part was made in the chemical department.* The work appeared to be desirable in view of the increasing attention which was then being paid to the physical history of the ocean, rendering exact knowledge of the relations of sea-water to heat necessary. With the exception of the imperfect observations of Münke, made nearly 50 years previously, the only attempt to determine the law of the thermal expansion of sea-water was due to Hubbard, of the United States National Observatory. The inquiry, of which the mathematical part was undertaken by Rücker, served to furnish all the data necessary to calculate the specific gravity of sea-water of any degree of salinity at any temperature between the natural limits.

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* 'Coal: Its History and Uses,' by Profs. Green, Miall, Thorpe, Rücker, and Marshall. Edited by Prof. Thorpe. Macmillan and Co. 1878.

In 1879 he contributed a short paper to the Physical Society (published in the 'Phil. Mag.' for 1880), "On a Suggestion as to the Constitution of Chlorine offered by the Dynamical Theory of Gases," in which he drew attention to the fact that the application of the theory of gases to the specific heats of a large number of substances, determined as far as might be under similar circumstances, leads to the alternatives that the atoms of which a molecule of free chlorine is composed are either less strongly united or are more numerous than in the case of other elements—a conclusion wholly independent of Victor Meyer's observations on the vapour density of chlorine which had then been recently published.

Rücker's work on the thermal expansion of sea-water caused him to take a practical interest in the general subject of thermometry, and at the York meeting of the British Association (1881), he read a joint paper "On the Calibration of Mercurial Thermometers,"† which led to the formation of a small committee, of which the late Prof. Balfour Stewart was Chairman, to inquire and report on the various methods which had up to that time been suggested for the purpose.

The report was presented in the following year and appears in the volume for 1882.‡ Allied to this subject were two short papers contributed to the

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* "On the Expansion of Sea-Water by Heat," by T. E. Thorpe and A. W. Rücker, 'Phil. Trans.,' vol. 166, p. 405 (1876).

† "On the Calibration of Mercurial Thermometers," by T. E. Thorpe and A. W. Rücker, 'Rep. Brit. Assoc.,' 1881.

‡ "Report of the Committee on the Methods Employed in the Calibration of Mercurial Thermometers" (Secretary, A. W. Rücker), 'Rep. Brit. Assoc.,' 1882.

'Phil. Mag.' for 1881, consisting of a critical examination of methods of correcting for the exposed thread, and for the movement of the zero with time and temperature, proposed by Dr. Mills,* in which it was sought to show that the contention that each thermometer requires its own independent equation for exposure-correction is invalid.

Two short joint papers, (1) "On a Relation between the Critical Temperatures of Bodies and their Thermal Expansions as Liquids,"† and (2) "On the Critical Temperature of Heptane,"‡ were communicated to the Chemical Society in 1884. Mendelejeff had proved in a paper contributed to the Society in the same year, from observations published by Thorpe in 1880, that the expansion of a liquid can be expressed by a very simple formula containing only one constant, which is different for different liquids. In the first of the papers above referred to the authors showed, (1) by applying van der Waal's theory to this result, that the reciprocal of the constant is the number obtained by subtracting 273 from the product of the critical temperature into a quantity which should be the same for all substances; and (2) the values of this quantity a obtained from different substances, for which the required experimental data exist, and to which Mendelejeff's formula applies, are found to be approximately constant and approach very closely to 2; and they concluded that if further investigation proves that the range of variation in the value of a is small, the formula affords a ready means of calculating the critical temperatures of bodies from their thermal expansions as liquids. In the second paper it was pointed out that from measurements of the relation of surface tension to temperature of the hydrocarbon heptane from *Pinus sabiniana*, made by one of the authors, its critical temperature was 281°. This value agrees closely with that deduced from Pawlewski's observation that there is a constant difference between the critical temperature and boiling point of strictly homologous compounds. Normal heptane boils under the standard pressure at 98·4°. This number added to Pawlewski's constant in the case of the paraffins, viz. 182·3°, affords 280·7° as the critical temperature of this hydrocarbon, in good agreement with the value deduced from surface tension observations.

In the spring of 1885 Rücker contributed a mathematical paper to the Physical Society on the theory of the self-regulating power of the compound dynamo.§ The method and chief conclusions in the paper had already been part of his teaching to his senior class in technical electricity. During the interval between the reception of his paper by the Society and its publication some months afterwards, Fröhlich had communicated the results of a similar investigation to the 'Elektrotechnische Zeitschrift.' Although Rücker's results were obviously independently obtained, with his characteristic courtesy he at once withdrew any claim to priority.

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* "Remarks on Dr. Mills' Researches on Thermometry," by T. E. Thorpe and A. W. Rücker, 'Phil. Mag.,' July and September, 1881.

† "On a Relation between the Critical Temperatures of Bodies and their Thermal Expansions as Liquids," by T. E. Thorpe and A. W. Rücker, 'Chem. Soc. Trans.,' 1884, p. 135.

‡ "On the Critical Temperature of Heptane," by T. E. Thorpe and A. W. Rücker, 'Chem. Soc. Trans.,' 1884, p. 165.

§ "On the Self-Regulation of the Compound Dynamo," by A. W. Rücker, 'Phil. Mag.,' 1885, p. 462.

With the exception of papers in conjunction with Prof. Reinold on "Liquid Films," and certain joint papers preliminary to the magnetic survey of the British Isles, which will be referred to subsequently, the foregoing account is a bald summary of what Rücker was able to accomplish in regard to published scientific inquiry during the eleven years of his stay in Leeds.

To the great regret of his colleagues and of the governing body he now announced his intention of resigning his professorship at the Yorkshire College. His action was determined, partly by family reasons, which led him to wish to reside near London—he had lost his wife after a short two years of wedded life, after the birth of a daughter—and partly because the metropolis seemed to offer a wider, if not a more promising, sphere for his activities. He had no intention, at the time, of abandoning a career in science. There was some suggestion that he might be able to undertake the Secretaryship of the British Association, and he contemplated the continuance of his work with Prof. Reinold, the experimental part of which could not readily be done at Leeds, for reasons already given.

When, however, it was known that he was about to leave Leeds and to relinquish teaching, an influential section of the Liberal party-managers in the town, in view of the Parliamentary Election then pending, conceived the idea of inviting him to contest, in the Liberal interest, one of the Conservative strongholds, then held by the local leader of that party, a wealthy and popular manufacturer, now Lord Allerton. Rücker was known to be what was called a "Moderate Liberal," but he had taken no active part in the political life of the town; he was in no sense a party man, and, indeed, had something approaching to contempt for the falsity of political extremes.

To put forward a politically unknown and untried candidate in a community where political feeling is apt to run high seemed a hazardous experiment, and at first was viewed with something like indifference, as a foregone conclusion, by the electorate of both sides. But those who were responsible for the attempt had confidence in their man. They were well aware of his general popularity, and trusted in his ability to make a favourable impression. With the political issues at that time before the country, Rücker was well able to hold his own, and he handled them with a skill, intelligence, and vigour that astonished and delighted his friends. He gradually won over a large proportion of that section of the voters who, as in every Election, are more attracted by men than by measures. After a series of successful gatherings, his final meeting, on the eve of the polling, culminated in a scene of enthusiasm and excitement such as even Leeds has seldom witnessed. It was a most strenuous struggle, altogether unprecedented in the division. In the end the Liberals were beaten by 267 on a total poll of nearly 9000; it was, no doubt, a political defeat for them, but it was no less a personal triumph for their candidate.

The following year brought the great disruption, but Rücker had no sympathy with the sudden *volte-face* of the party. He was invited to fight the Pudsey Division of Yorkshire as a Liberal Unionist, but again failed to secure election. Science gained thereby, but the Legislature, no less certainly, lost one who would have been an ornament to its body.

With his young daughter Rücker now settled at Clapham Park, in the home of his parents. He continued his scientific work, and became a regular attendant at the meetings of the Royal and Physical Societies. His colleague at Leeds, Prof. Thorpe, had followed him to London in 1885 as successor to the late Sir Edward Frankland at the Normal School of Science and Royal School of Mines. In the following year the Chair of Physics at South Kensington became vacant by the death of Dr. Guthrie, and Rücker was induced to reconsider his decision to abandon the career of a teacher and to offer himself as a candidate for the Professorship. He was nominated with the cordial concurrence of the late Prof. Huxley, then Dean of the School, and the other members of the Council, and the nomination was confirmed by the authorities of the Science and Art Department. Rücker, who was now 38 years of age and in the full tide of his intellectual vigour, brought to the discharge of his new office all the energy and ability that had characterised his work at Leeds, with the added knowledge, skill, and experience of a dozen years of active teaching. He found in London an eager, enthusiastic body of students, and he gave them of his best. He practically reorganised the work of the department. With an ample command of apparatus, he took full opportunity of the resources of the Science Museum, and probably no course of experimental lectures in physics was ever more admirably illustrated. As an instance of the pains he spent in the preparation of these lectures, his method of optically demonstrating electrical stress, based on Dr. Kerr's well-known experiments, which he devised in concert with Mr. C. V. Boys, may be mentioned. This method, with a series of experiments illustrating the work of Kerr and Quincke, was published in the 'Journal of the Society of Telegraph Engineers,' p. 72, vol. 17 (1888), and was made the subject of a Friday evening discourse at the Royal Institution in 1889.

In his advanced lectures he took his hearers far beyond the limits of ordinary text-book knowledge. His treatment, as might be expected, was largely mathematical, and his insistence on a proper mathematical equipment on the part of his auditors had considerable influence on the character of the mathematical teaching in the School. He gave a special course on the mathematical methods of especial value in Physics.

The structural arrangements in the old building of the Normal School, which had not been designed for instruction in physical science, were ill adapted to the needs of a modern physical laboratory, but with the help of a body of able and loyal assistants, mostly his senior students, he instituted a good and comprehensive course of practical instruction. The knowledge and experience he thus gained in working a practical course in physics are reflected in the present admirable physical laboratories of the Imperial College of Science and Technology, the design and arrangements of which were wholly inspired by him.

During the early years of his connection with the South Kensington College, most of the time he could spare for original inquiry was devoted to the magnetic survey of the British Isles. He was, however, able to complete in his first year, a second instalment of his work with Prof. Reinold on "Liquid Films." The results of this inquiry, which extended over many years, are contained in the following papers:—

"On Black Soap Films." By A. W. Rücker, 'Nature,' vol. 16, pp. 331–333 (1877).

"On the Thickness of Soap Films." By A. W. Reinold and A. W. Rücker, 'Roy. Soc. Proc.,' vol. 26, pp. 334–345 (1877).

"On the Electrical Resistance of Thin Liquid Films, with a Revision of Newton's Table of Colours." By A. W. Reinold and A. W. Rücker, 'Roy. Soc. Proc.,' vol. 31, pp. 524–525 (1881); 'Phil. Trans.,' vol. 172, pp. 447–489 (1882).

"On the Limiting Thickness of Liquid Films." By A. W. Reinold and A. W. Rücker, 'Roy. Soc. Proc.,' vol. 35, pp. 149–151 (1883); 'Phil. Trans.,' vol. 174, pp. 645–662 (1884).

"Liquid Films and Molecular Magnitudes." By A. W. Reinold and A. W. Rücker, 'Nature,' vol. 28, pp. 389–393 (1883).

"On the Influence of an Electric Current on the Rate of Thinning of a Liquid Film." By A. W. Reinold and A. W. Rücker, 'Phil. Mag.,' January, 1885, pp. 94–100.

"On a Relation Between the Thickness and Surface Tension of Liquid Films." By A. W. Reinold and A. W. Rücker, 'Roy. Soc. Proc.,' vol. 40, pp. 441–445 (1886); 'Phil. Trans.,' vol. 177, pp. 627–684 (1886).

"On the Critical Mean Curvature of Liquid Surfaces of Revolution." By A. W. Rücker, 'Phil. Mag.,' January, 1887, p. 35.

"On the Thickness and Electrical Resistance of Thin Liquid Films." By A. W. Reinold and A. W. Rücker, 'Roy. Soc. Proc.,' 1883; 'Phil. Trans.,' vol. 184, pp. 505–529 (1893).

Prof. Reinold writes with reference to this work:

"Some short time after Rücker's election to the Professorship of Physics in the Yorkshire College he proposed that we should make some experiments together on thin soap films, and especially on such as were sufficiently thin to exhibit the black of the first order of Newton's scale of colours. He was greatly interested in Plateau's work and thought that further experiments might yield important information with regard to molecular distances.

"There were difficulties in the way of joint work, as he was established in

Leeds and I in Greenwich. However it was arranged that apparatus should be set up at Greenwich and that we should carry out experiments in common as opportunity offered during vacations. The first experiments were made in 1875 and were continued from time to time during the years Rücker was at the Yorkshire College, and afterwards when he was Professor at the Royal College of Science.

"After many fruitless experiments, a method was devised which subsequently gave good results. A cylindrical film was formed between two platinum rings, one vertically above the other, and was allowed to thin. The thicknesses of the successive coloured bands were obtained from Newton's scale, and the electric resistance of the film was measured by Wheatstone's bridge. The resistance of the black, when black was formed, was taken to be the difference between the resistance of the whole film and that of the coloured portion. The liquid from which the films were formed was Plateau's *liquide glycérique*, with 3 per cent. KNO_3 added to improve the conductivity. The results were published in the 'Proceedings,' 1877, and were summarised as follows:—

"(1) The variations in the thickness of the black portions of the films were but a small fraction of that thickness.

"(2) The thickness was independent of the breadth of the black rings.

"(3) It was also independent of that portion of the film which appeared to the naked eye to be in contact with it. It was also shown that, on the assumption that the specific resistance of the liquid in the film was identical with that of the same liquid in the mass, the average thickness of the black films observed must have been 12 micromillimetres.

"Improved apparatus was then employed, and experiments were carried out with the object of testing the assumption made with regard to the constancy of the specific conductivity of the liquid. These were described in a paper published in the 'Phil. Trans.' for 1881, entitled, "On the Electrical Resistance of Thin Liquid Films, with a Revision of Newton's Table of Colours." The films were cylinders as before. The thicknesses of the successive coloured bands were estimated by the colours they exhibited when seen by light reflected at a definite angle. Two different angles of reflection were used, and thus it was possible to check and extend the numbers given in Newton's scale. The result of the experiments showed that for thicknesses greater than 374 micromillimetres the assumption that the specific resistance of the liquid of the films was identical with that of the liquid in mass was justified. But for thicknesses less than 374 $\mu\mu$ the experiments yielded no results from which trustworthy conclusions could be drawn; hence the desirability of some independent method of experiment. It had been observed that plane circular films formed in a glass tube thin very readily to the black, and although it was probably impossible to measure the thickness of a single film by any optical method, it might nevertheless be possible to determine the total thickness of a number of parallel films. R. and R. succeeded in doing this. The tube with its contents was placed in an apparatus for producing interference of thick plates. One of the interfering rays passed through the tube. A piece of a steel knitting-needle was included within it. When the films became black a number of them were broken by moving the needle by a magnet, and the thickness could be calculated by observing the positions of the interference fringes before and after the rupture. By this method the mean thickness of the films was measured on the assumption that the refraction index of the films was the same as that of the liquid in mass.

"Concurrently with the optical experiments, the thickness of a black cylindrical film was measured electrically as follows:—

"An electric current was passed through the film from top to bottom and the difference of potential between two gold wires inserted in the black, one above the other, and about 5 mm. apart, was compared with the difference of potential between two points in the circuit separated by a known resistance. The thickness of the black was hence deduced, on the assumption that the specific resistance of the film is the same as that of the liquid in mass.

"The two methods yielded results in most satisfactory accord. The mean of all the electric observations gave a thickness of 11·8 $\mu\mu$, that of all the optical 11·4 $\mu\mu$, an agreement which placed it beyond doubt that the mean value for all the films observed was really about 11·6 $\mu\mu$.

"In 1884–5–6 a number of experiments were carried out with the object of determining whether there is any change in the surface tension of a film as it becomes thinner. An account of these was published in the 'Phil. Trans.,' 1886. The method consisted in measuring the changes of curvature of two films in communication with each other as one became much thinner than the other, the forms of the films being originally cylinders, or one a cylinder, and the other a sphere. The theory of the method as well as that of the methods adopted by Plateau and Lüdtge was fully discussed by Rücker. The general result of this inquiry was expressed as follows—when the black part of a soap film forms in a normal way, no evidence of any change of surface tension dependent on the thickness of the film is furnished by a direct comparison of the tensions of thin and thick films over a range of thickness extending from 1350 to 12 micromillimetres.

"This conclusion was based upon a method of experiment by which a change of $\frac{1}{2}$ per cent. in the value of the tension must have been detected, had it existed. With regard to the magnitude of the radius of molecular action, it was asserted that the evidence (furnished by these experiments) in favour of its being less than 6 $\mu\mu$ (half the thickness of an ordinary black film) is far stronger than that on which the frequently quoted statement of Plateau that it is less than 50 $\mu\mu$, is based.

"The paper published in the 'Phil. Trans.,' 1893, dealt with the results of experiments on films formed of soap solutions containing smaller amounts of metallic salt than those whose properties had previously been studied. The optical method showed that the real thickness of black films increases from

$12\,\mu\mu$ to about $30\,\mu\mu$ as the solutions are more dilute and the amount of added salt less. Under the same conditions the thicknesses, as decided from the electrical experiments, were far greater than the greatest thickness at which a film can appear black. In such case, therefore, the electrical method does not give the true thickness of the film, and the hypothesis that the specific conductivities of the film and of the liquid in mass are the same, is untenable.

"Theories which might account for this change of conductivity were discussed."

The bearing of this work on molecular dynamics and on the theory of the constitution of matter was explained by Rücker in an address "On the Range of Molecular Forces," that he was induced to deliver in 1888 to the Chemical Society, in which with admirable clearness he summarised all the attempts made up to that time to obtain limiting values of molecular magnitudes ('Chem. Soc. Trans.,' 1888, vol. 53, p. 222).

In the same year he contributed a mathematical paper to the Physical Society, "On the Suppressed Dimensions of Physical Quantities" ('Phil. Mag.,' 1889). In this he advocated the introduction of symbols for what he called secondary fundamental units instead of suppressing their dimensions in the calculation of physical quantities, where we not unfrequently arrive at indeterminate equations in which two or more unknowns are involved. In such a case an assumption has to be made, usually an abstract number, leading to dimensions of dependent units which are obviously artificial, and often unintelligible, inasmuch as they do not necessarily indicate their true relations to length, mass, and time. The paper, which appears to have been suggested by difficulties he had met with in teaching, contains a number of examples which seemed to justify the change in the method of exhibiting the dimensions of electrical quantities, apart from the great theoretical advantage of being able to write electrostatic and electromagnetic quantities as being of the same absolute dimensions.

In 1891 he published the results of a series of determinations made with his colleague, the late Sir William Roberts-Austen, "On the Specific Heat of Basalt" ('Phil. Mag.,' October, 1891), in compliance with the request of the Rev. O. Fisher. The observations showed that whilst the mean specific heat of basalt increases regularly with the temperature, there is a large absorption of heat in the neighbourhood of 800°, which raises the mean specific heat between 750° and 880° to the large value of 0·636—nearly three times greater than the value below 750°.

The extraordinarily rapid development of the technical applications of electricity in our towns, especially in regard to lighting and traction, threatened at one time to introduce very serious domestic troubles, owing to leakage in currents, proximity of dynamos, etc., and it greatly affected the work of physical laboratories and magnetic observatories. It was necessary, for example, to protect the absolute magnetic instruments at Greenwich by means of screens—from disturbances due to a neighbouring dynamo, and the electric tramway lines in the vicinity so seriously affected the magnetic instruments in the Kew Observatory as to necessitate their removal to Eskdalemuir.

As Rücker subsequently wrote: "From all parts of the world we hear of observatories ruined or threatened by the invasion of the electrical engineer. Toronto and Washington have already succumbed, Potsdam, Parc St. Maur, Greenwich, and Kew are besieged, and the issue largely depends upon whether these great national observatories can or cannot make good their defence.

"It seems to be a law of Nature, ruling alike the human race and the humblest microbe, that the products of an organism are fatal to itself. The pessimist might infer that we are in the presence of another instance of the universality of the application of this law, and that pure science is threatened by the very success of its practical applications. The smoke of our cities blots the stars from the vision of the astronomer, who, like the Anchorites of old, flies from the world to mountains and desert places. It is only in the small hours of the morning, when,

'Save pale recluse, for knowledge seeking,
All mortal things to sleep are given,'

that the physicist can escape from the tremors of the traffic of a great town. And now the science of Terrestrial Magnetism which, on the one hand, is forging another link to connect the sun and the earth, and, on the other, is penetrating within the surface of the globe to depths beyond the ken of the geologist, is threatened by the artificial earth currents of the electric railway . . . We are not so foolish as to desire to embark on a crusade against a great industrial improvement, of which science may well be proud; on the other hand, we must hold fast to the position that provision for the conveniences which are immediately appreciated by the public should be made with as little damage as possible to those studies which are not less for the benefit of the race."

In 1893 Rücker communicated a paper to the Physical Society "On the Magnetic Shielding of Concentric Spherical Shells" ('Phil. Mag.,' January, 1894–5), in which he discussed the best conditions for shielding. His attention was again directed to the subject when engaged in considering the arrangements of the new physical laboratories of the Imperial College of Science and Technology, and in the course of an inquiry on the magnetic field produced by electric tramways. In a paper read to the Physical Society ('Proc. Phys. Soc.,' vol. 17 (1900); 'Phil. Mag.,' April, 1901) he drew attention to the fact that the leakage currents on a homogeneous earth affect directly only the horizontal force, while the vertical disturbing force is due only to the difference of the effects of the currents in the trolley wires and rails or other horizontal conductors by which the current is conveyed to and from the cars. If the return conductors are insulated and parallel to the trolley wires, the outgoing and returning horizontal currents are equal (since there is no leak), and the effects are zero at any point in the same horizontal plane as the rails, the distance of which from the line is considerable with respect to the height of the trolley wires above the ground.

But Rücker's chief service to science was his share in planning and prosecuting the magnetic survey of the British Isles, already referred to—a work extending over a dozen years and which occupied much of his energy and thoughts during that time. His interest in this subject was secured by a very simple circumstance. The writer of this notice being about to visit Colorado for the purpose of observing the total solar eclipse of July, 1878, undertook, at the suggestion of the late Mr. Whipple, of the Kew Observatory, and with the concurrence of Prof. Balfour Stewart, who offered to lend the necessary instruments, to make observations of the magnetic elements along the fortieth parallel of latitude between the Atlantic Ocean and the Great Salt Lake, Utah. At that time over the immense tract of country lying between the Mississippi and the Pacific Ocean, there was only a single determination of a magnetic element indicated on Sir E. Sabine's maps—viz. an approximate estimation of declination made for the purpose of orienting the Mormon Temple at Salt Lake City. These observations, in which the writer was assisted by Dr. Schuster, were published by the Royal Society.*

Rücker's interest in them led him to consider favourably a suggestion that he should associate himself with the writer in a magnetic survey of the United Kingdom, then being contemplated by the Kew Committee. Although a scheme was submitted involving the co-operation of Dr. Balfour Stewart, Father Perry, Prof. Fitzgerald, and certain of the Kew observers, no steps were taken, on the ground that it was the desire of the then Chief Hydrographer that the work should be undertaken by his Department. No action, however, was taken by the Admiralty. Accordingly, Profs. Rücker and Thorpe suggested that they might make a beginning on the West Coast of Scotland, a section that had been assigned to them on the contemplated scheme, for the reason that one of them was familiar with the district and had the means of cruising amongst its numerous islands and sea-lochs. There was a special reason for making a commencement in Scotland, as that portion of Great Britain had been more fully and more systematically surveyed than any other, namely by Sabine in 1834–38 and by Welsh in 1857–58, and might presumably therefore afford more accurate data for the discussion of secular changes, in spite of much disturbed ground. To begin with, it was decided to ascertain more precisely the seat of a large local disturbance in the island of Mull, which had been indicated by Balfour Stewart who reduced and discussed Welsh's Scotch observations ; to fix, if possible, the position of maximum disturbance; and to determine the influence of the area on the direction of the isoclinal lines. The existence of this area of local attraction was not only confirmed, but its centre or focus was more accurately ascertained. It was further found that, however great may be the absolute mass of the magnetic matter in Mull, its influence extends to no very great distance from the island, and hence the determination of the normal direction of the isoclinal line in the neighbourhood is not likely to be affected materially. The results of this work, which occupied the September of 1883, were communicated to the Royal Society.*

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* "A Magnetic Survey of the Fortieth Parallel in North America, between the Atlantic Ocean and the Great Salt Lake, Utah," by T. E. Thorpe, 'Roy. Soc. Proc.,' vol. 30, p. 132.

The observations were resumed in the following summer upon all the magnetic elements at a number of stations in the western half of Scotland. As the Kew Committee were still not in a position to take action, it was decided in 1885 to extend the work to the eastern half of Scotland, and a preliminary statement of the results was communicated to the British Association at York.†

The observers were then induced to include the rest of the United Kingdom, and by the end of the autumn of 1888 the survey was completed. Observations of all the magnetic elements were made at about 200 stations at an average distance apart in normal districts of about 30 miles, and the results were reduced to the epoch January 1, 1886.‡

The memoir, when presented to the Royal Society in 1889, was made the Bakerian Lecture of the year, the description of its contents being given by Rücker, by whom it was mainly drawn up. It differed from the accounts of previous surveys in the mode of presenting the results, whereby a greater test of their accuracy was afforded; the validity of the method of correcting for diurnal variation and disturbance was reconsidered, and, what perhaps was the most striking innovation, far more attention was given to the distribution and causes of local magnetic attraction.

A fuller investigation of certain of these disturbed areas seemed desirable. It had been found that powerful horizontal disturbing forces acted westwards from the Sound of Islay, from Iona, and from Tiree, as well as at Glenmorven, as indicated by Welsh's survey. If the general westward tendency of the horizontal disturbing forces was due to some source of error, the extreme south of the Hebrides would probably be similarly affected. If, on the other hand, the directions of the forces were due to a physical cause such as a centre of attraction out at sea to the west of Tiree, then the disturbing forces in the Southern Hebrides would almost certainly be directed southwards towards it. Moreover, on general grounds, it was desirable to multiply stations on the edge of the area of the survey, as the normal values of the elements are especially uncertain at these points.

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* "Note on the Irregularities in Magnetic Inclination on the West Coast of Scotland," by T. E. Thorpe and A. W. Rücker, 'Roy. Soc. Proc.,' vol. 36, pp. 5–10 (1883).

† "On the Third Magnetic Survey of Scotland," by T. E. Thorpe and A. W. Rücker, 'Brit. Assoc. Rep.,' 1885.

‡ "A Magnetic Survey of the British Isles for the Epoch January 1, 1886," by A. W. Rücker and T. E. Thorpe, 'Phil. Trans.,' vol. 181, pp. 53–328 (1890).

It had been further observed that a locus of attraction probably existed within an area 150 miles long and 40 miles broad, in Yorkshire and Lincolnshire, which it seemed advisable to define more accurately.

Accordingly, one of the observers undertook to carry out a second series of measurements among the Western Isles and Outer Hebrides during the summer of 1889, whilst the other ran a chain of thirty-five additional stations through Lincolnshire and Yorkshire. The results of the first Scotch series showed: (1) that the direction of the disturbing horizontal force at Bernera, which is the southernmost island of the Hebridean group, is due south; and (2) that as this point is approached from the north the downward vertical disturbing attraction on the north pole of the needle regularly increases, which exactly agrees with the supposition that a centre of attraction is being approached, and that this centre is to the south of the Hebrides and to the west of Tiree. The second series indicated: (1) that the horizontal disturbing forces tended towards the centre of the district; and (2) that the downward vertical disturbing forces are greater in the centre of the district than at its boundaries, with well-marked regions of high vertical force at Market Weighton, where the older sedimentary rocks are known to approach the surface, and at Harrogate, which is on the apex of an anticlinal. The existence of the line of attraction was thus definitely established, and for above 90 miles its position was ascertained to within 5 miles.*

The significance of these results as confirming and extending the deductions from the survey concerning the distribution of magnetic material beneath the earth's surface, pointed to the desirability of a still more detailed investigation, and accordingly, with the help of a liberal allowance from the Government Grant Fund, a new survey was undertaken involving observations at 677 additional stations. As it was important that the observational work should be concluded as rapidly as possible in order to avoid complications due to uncertainties in secular change, Rücker trained three of his assistants in the Physical Department of the Royal College of Science in the use of the instruments and in the methods of direction. In all, therefore, five observers were engaged upon the work, which occupied the spring and summer of 1890, 1891 and 1892.

The results were reduced to the epoch January 1, 1891, and included the observations of the previous survey. The conclusions of the second survey are therefore based upon the results obtained at 882 stations distributed throughout the British Isles. As before, the greater part of the work of reduction and tabulation was performed by Rücker and his assistants, and he undertook the chief labour of tabulating and arranging the material.

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* "Preliminary Note on Supplementary Magnetic Surveys of Special Districts in the British Isles," by A. W. Rücker and T. E. Thorpe, 'Roy. Soc. Proc.,' vol. 47, pp. 443–445 (1890).

The memoir was presented to the Royal Society in 1894, and with its maps and plates constitutes vol. 188 of the 'Phil. Trans.'*

This work not only served to fix with precision the direction of the isomagnetics for the particular epoch selected, and for a reasonable period of time subsequently, but, what was even more important, it ascertained for the first time the existence, position, relative extent and characteristics of what may be called the magnetic districts of the United Kingdom, and indicated their relation to geological constitution.

In awarding Rücker a Royal Medal in 1891 for his work with Prof. Reinold on liquid films, and his contribution to the theory of direct-current dynamos and motors, and for his remarkably original treatment of the results of the magnetic survey of the British Isles, the President, Lord Kelvin (then Sir William Thomson), in regard to the last item, laid special stress on the exhaustive discussion of the observations in reference to regions of local magnetic disturbance and their relation to the geological constitution of the earth's crust in the neighbourhood.

In a work of this magnitude, extending over so many years, and occupying constant thought, there are necessarily many side-issues, and some of these engaged Rücker's attention. Experience with the Kew magnetometer in the field led to certain additions to the instrument which considerably facilitated its use in making declination observations, and formed the subject of a communication to the Physical Society in 1888, and this interest in the purely determinative side of magnetic work led him to institute a critical comparison of the English instruments with those used by M. Moureaux in the Magnetic Survey of France ('Rep. Brit. Assoc.,' 1889).†

The two main theories which have been proposed to account for local magnetic perturbations are earth currents and the presence of visible or concealed magnetic rocks. Both causes of the observed effects may co-exist, but of the two the rock-magnetism theory would seem to be the more important. Rücker was led to this conclusion by facts which are set out at length in the accounts of the two surveys, and he elaborated in various lectures and in certain popular science articles he wrote at about this period, the view that the presence of magnetic matter, magnetised by induction in the earth's field, is the principal cause of the existence of the magnetic ridge-lines and foci of attraction which he had so carefully traced.

In a paper "On the Relation between the Magnetic Permeability of Rocks and Regional Magnetic Disturbances," ('Roy. Soc. Proc.,' vol. 48, pp. 505–535, (1890)), he gave a series of determinations of the magnetic susceptibility of a number of rock specimens, and discussed the order of magnitude of the magnetic disturbances that the presence of such rocks in the earth's magnetic field would produce. This investigation, in spite of uncertainty in regard to details, afforded, for the first time, a definite answer to the inquiry whether the mere presence in the earth's magnetic field of concealed magnetic rocks, such as those which exist on the surface, and of which the permeabilities had been measured, would suffice to account for the observed local or regional magnetic disturbances in districts where the superficial deposits are non-magnetic. He subsequently extended the measurements so as to include the susceptibilities of a number of Australian basalts, and communicated the results to the Royal Society of N. S. Wales in 1894. A further contribution to the subject was made in 1898, when he published the results of a large number of measurements of the magnetic susceptibility of typical British basalt rocks, made with the assistance of Mr. W. H. White, in an improved form of the original apparatus.*

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* "A Magnetic Survey of the British Isles for the Epoch January 1, 1891," by A. W. Rücker and T. E. Thorpe, 'Phil. Trans.,' vol. 188, pp. 1–661 (1896).

† "Note on Some Additions to the Kew Magnetometer," by T. E. Thorpe and A. W. Rücker, 'Phil. Mag.,' August, 1888, pp. 122–126.

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The data afforded by the two surveys were employed by Rücker to ascertain the probability of the existence of vertical earth-air electric currents in the United Kingdom, and the results of the inquiry, which led to the conclusion that such currents were non-existent, were communicated to the Physical Society in 1895.†

Rücker's work on terrestrial magnetism secured for him an international reputation which was fittingly recognised when he was made President of the Permanent Committee on Terrestrial Magnetism and Atmospheric Electricity of the International Meteorological Conference appointed at Paris in 1896, and he occupied the Chair when the International Conference on Terrestrial Magnetism was held in connection with the meeting of the British Association at Bristol in 1898.

Rücker was elected into the Royal Society in 1884, and was a Member of Council in 1887–89 and again in 1894. In 1896 he became one of the Secretaries in association with the late Sir Michael Foster. He held the office for five years, conducting the affairs of the Society, as the Council gratefully recognised, "with exceptional skill, energy, and tact." He had acted as Deputy Chairman of Lord Rayleigh's Committee which preceded the creation of the National Physical Laboratory, and it fell to his duty as Secretary to conduct with the Treasury the negotiations that resulted in its foundation, and to draft the rules under which it was to be worked. He was on its Executive Committee and, so long as the state of his health permitted, took an active interest in its progress. He took a leading part in the organisation of the 'International Catalogue of Scientific Literature.'

He was further concerned in the delicate negotiations with the Geographical Society in connection with Captain Scott's first Antarctic Expedition. Captain Scott showed his appreciation of these services by conferring his name on a mountain in the Royal Society Range, adjacent to Mount Hooker. In 1908 he was again elected to the Council, but retired in 1909, as his engagements precluded his regular attendance. He represented the Royal Society on the Governing Body of Rugby School until 1912, when he resigned after going to Everington, in order, as a local duty, to take part in the management of Bradfield College.

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* "On the Determination of the Magnetic Susceptibility of Rocks," by A. W. Rücker and W. H. White, 'Roy. Soc. Proc.,' vol. 63, pp. 460–466 (1898).

† "On the Existence of Vertical Earth-Air Electric Currents in the United Kingdom," by A. W. Rücker, 'Phil. Mag.,' February, 1896, pp. 99–106.

Rücker joined the Royal Institution in 1886, and remained a Member for 29 years. He delivered a series of lectures on Molecular Forces in 1887, and gave a course of juvenile lectures on Electricity during the Christmas vacation of 1889–1890. His remarkable powers of lucid exposition made him very popular as a lecturer, and his Friday evening discourses were always attractive and interesting on account of their novelty and as records of personal achievement. They were on "Liquid Films" (1885), "Electrical Stress" (1889), "Magnetic Rocks" (1891), and "Earth Currents and Electrical Traction" (1899). On March 8, 1895, he gave a discourse on "The Physical Work of von Helmholtz," who had died exactly six months previously, in which he sought to convey within the inexorable limitation of time which custom imposes, some idea of the multifarious activities of that gifted man. Rücker on this occasion for the first time exhibited an apparatus which he had devised, in concert with Mr. Edser, one of his assistants, to demonstrate the objective existence of combination-tones as upheld in part by Helmholtz in the 'Lehre von den Tonempfindungen,' but denied by König, Bosanquet, and Preyer. Evidence of the objective character of these tones had been previously obtained by Lummer by means of the microphone, but Rücker's experiments were made in ignorance of that fact, and by wholly independent methods. An account of these experiments was subsequently communicated to the Physical Society.*

The admirable obituary notice in the Proceedings ('Roy. Soc. Proc.,' vol. 59, pp. xvii–xxx) of von Helmholtz, who was a Foreign Member of the Royal Society, and was awarded the Copley Medal in 1893, was written by Rücker.

He was a regular attendant at the meetings of the British Association and took an active part in its proceedings, especially in those of Section A. In 1891 he was appointed General Treasurer, and in 1898 a Trustee. He presided over the Mathematical and Physical Section at the Oxford meeting of 1894. A part of his address was concerned with a matter which was then uppermost in his mind, namely, the want of uniformity in the indications of the instruments employed in observations of terrestrial magnetism—not only in instruments of different type, but in those of identical pattern.

In 1900 Rücker was nominated as President for the ensuing Glasgow meeting. This meeting was made memorable by the address with which it was inaugurated. It differed in type from the greater number of its predecessors in that, apart from certain timely and graceful references to current topics of general and local interest, it was practically confined to a single theme, namely, a review, set out with the impartiality of a judicial summing-up, of the case for and against the validity of the doctrine of atoms. This address was a fitting ending to a strenuous career of high endeavour and rich achievement in science. In its wealth of detail, in the soundness and closeness of its argument, in its orderly presentation of statement, and in its grace and charm of expression, it was unquestionably the most highly polished, as it was one of the most successful, of his literary efforts.

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* "On the Objective Reality of Combination Tones," by A. W. Rücker and E. Edser, 'Phil. Mag.,' April, 1895, pp. 341–357.

Rücker's skill in administration, his academic experience and well-tried capacity, combined with his urbanity and courtly bearing and acknowledged standing in the scientific world, marked him as eminently qualified to direct the affairs of a modern University. He might have remained at Leeds as its first Principal and Vice-Chancellor had he so chosen. Liverpool and Birmingham in turn had offered him the headship of those Universities.

In 1901 he was induced to consider the expediency of accepting the Principalship of the reorganised University of London. It was confessedly an onerous and difficult position, but a variety of circumstances led him, after much hesitation, to accede to the request, not the least powerful of which was his loyalty to the cause of University education in London. He had been a member of the old Senate in Burlington Gardens since 1892, and was fully acquainted with the working of the University in those days, its traditions, aims, and policy, and he had taken an effective share in the deliberations preceding the establishment of the Royal and Statutory Commissions, which fundamentally changed its constitution. He had been, moreover, an active agent under the leadership of Huxley, in the movement for the establishment of the so-called professorial University of London, and was one of those who succeeded in persuading the members of the old Senate to accede to the principle of enlarging the functions of the University, so as to include teaching by persons attached to its own body, in addition to its duties as an examining board. Rücker's well-known views as to the true character of a great Metropolitan University undoubtedly rendered the prospect of his Principalship unwelcome to a certain section of the newly constituted Senate, and, as the late Sir Henry Roscoe, who was then Vice-Chancellor, and who had, with some difficulty, induced Rücker to become a candidate, and who had most earnestly and warmly advocated his claims before the Senate and its committees, states in his 'Life and Experiences,' he only gained his cause "after long and serious opposition." And he adds: "The result of Sir Arthur Rücker's appointment has more than justified my efforts, and has gained the appreciation of all interested in the University—in fact, I consider that my success in obtaining his appointment as Principal was the best day's work I ever did for the University."

It is impossible here to go into any great detail of what Rücker was instrumental in achieving during the seven years of his Principalship. His successive annual reports contain the story of his efforts. His most striking contribution to the organisation of higher education in London was undoubtedly the timely and effective support he gave to the policy of incorporating the Greater London Colleges in the University itself. The Act of 1898 left the University without control of finances and with only a very limited control of the educational policy, of the institutions connected with it, and correspondingly they were scarcely identified in the eyes of the public with the University. The authorities of University College had approached the Commissioners appointed under the Act with a view to being incorporated in the new University from the outset, but had been informed that the Commission had no power to entertain the proposal. When, soon after Rücker's appointment, the Drapers' Company decided to assist University College by paying off a debt of £30,000, he was informally consulted as to the conditions to be laid down, and the Company adopted his suggestion that the gift should be made as a step towards incorporation. Eventually over £200,000 was raised for the purpose. University College was incorporated on January 1, 1907, under an Act passed in 1905, and a similar Act providing for the incorporation of King's College was passed in August, 1908. The proposal in 1903 to develop and further co-ordinate the work of the science and engineering colleges at South Kensington (the Royal College of Science, the Royal School of Mines, and the Central Technical College of the City and Guilds Institute), which led to the creation of the Imperial College of Science and Technology, raised the problem of incorporation in an acute form. Rücker, as a member of the Departmental Committee of the Board of Education, on whose recommendation the Imperial College was founded, reported in a supplementary note signed by Lord Reay and himself in favour of union with the University. But there was serious opposition, and it was clear from the first that nothing could be done without another Royal Commission, and equally clear in 1908 that its appointment could not be long delayed. By that time he felt that he could not much longer stand the continual strain of heavy work in a somewhat anomalous position, and partly for that reason, and partly because he recognised that he had probably induced the Senate to pursue his policy as far as they were willing to go, and lastly because he wished to be free to place his views in full before the Commission, he decided in October, 1907, to resign as from the following September.

Rücker laid down his office amidst every mark of grateful appreciation and esteem. All those of the staff who were in close touch with him had for him the warmest affection and regard, for he was most generous in his appreciation of the work they did for him. They knew, as no one else could, how strenuous and single-hearted his work had been, and with what toil and sacrifice of leisure and health it had been accomplished. They gave utterance to their sentiments of friendship and regard in a happily phrased address accompanying the gift of a no less felicitously designed rose bowl, in which "the artists, faithful to truth as well as to beauty, have not forgotten to mingle with the roses on its sides a certain number of thorns."

Some reference is required to Rücker's work as Principal outside the University as well as within its walls. At conferences of science teachers, whether connected with the University or the Public Schools, it seemed almost inevitable that he should be called upon to preside. This was due not merely to the circumstance that he was Principal of the University, but to the fact of his well-known ability in the management of the business of a public meeting, his conspicuous impartiality as a chairman, and his power of evoking sound discussion.

He was necessarily brought into contact with the various polytechnics, technical institutes, medical schools, and other educational agencies in the London area, and his services were in constant requisition at prize-givings, speech days, and such like functions. Much of his time and energy were spent in the preparation and delivery of the addresses needed on these occasions, often at considerable personal inconvenience, and to the interruption of the consideration of important administrative matters at the University which would have seemed to have demanded all his thought.

Rücker's skill as an organiser was not less exercised by the visit in 1906 of the representatives of the French universities and of the Collège de France, as guests of the University of London—a gathering without precedent in the history of universities and a notable illustration of that *entente* which is destined to affect so profoundly the highest interests of humanity. In the following year the visit was returned and the friendship between the two great metropolitan universities still further cemented.

The academic world soon recognised Rücker's power as an educational force, and within the first few years of his Principalship each of the larger universities in the kingdom had elected him an honorary graduate of its body: he was a Doctor of Science of Oxford, Cambridge, Victoria, Leeds, and Belfast, and Doctor of Laws of Glasgow and Edinburgh. In 1902 he received the honour of knighthood. He was a member of the Departmental Committee on the Royal College of Science, 1904, which preceded the establishment of the Imperial College of Science and Technology. He served upon the Royal Commission on Irish Universities in 1906; on the University of Belfast Commission in 1908, in which year he was also placed on the Carnegie Trust for the Scottish Universities. In 1911 he became a member of the Royal Commission of the 1851 Exhibition and of the Advisory Committee of the Board of Education on University Grants. The idea of the federated superannuation system of 1913, now adopted by all the University Institutions in receipt of Exchequer grants, and certain other University Institutions, mainly originated with Rücker, who worked out the details of the scheme. In 1892 he was appointed by the Board of Trade a gas referee in succession to Prof. Tyndall, and in the same year he was made a member of the Board of Visitors of the Royal Observatory at Greenwich in the vacancy caused by the death of Sir George Airy. The

Board of Trade also appointed him in 1910 a member of the Committee on Sight Tests.

In 1892 Rücker married Thereza, third daughter of Mr. Nevil Story-Maskelyne, M.P., F.R.S., of Basset Down House, Swindon, grandson of the Rev. Nevil Maskelyne, the Astronomer-Royal. By this union he became connected with a family which, for three generations on both sides, had furnished Fellows to the Royal Society.

A Minute of Council of November 4, 1762, shows that Mr. Rücker, "an eminent merchant," negotiated a foreign exchange on behalf of the Society in connection with the expedition of Mason and Dixon to observe the transit of Venus in 1761. The "eminent merchant" here referred to was John Anthony Rücker, the founder of the London house, in which Rücker's father was a partner. In the same account appears the name of the Rev. Nevil Maskelyne, who was sent to St. Helena for the same purpose.

Rücker had been previously associated with Mr. Story-Maskelyne as an assistant in the Mineralogical Department of the British Museum, and had studied crystallography under him at Oxford. One son—Arthur Nevil Rücker—was born of this marriage in 1895; after serving at the War Office since the outbreak of the war, he now holds a temporary commission in His Majesty's Forces. Olive, the daughter of his first wife, married, in 1911, Mr. Reginald Hooker, fourth son of the late Sir Joseph Hooker, a former President of the Society.

On his resignation of the Principalship of the University, Rücker decided to give up the house in South Kensington which he had occupied since 1891, and to retire to the country. Although born and bred a Londoner, to spend the days of his well-earned leisure in the dignified seclusion of a club, even so well appointed as the Athenæum, had no attractions for him. A few years previous to his resignation he had rented Everington as a week-end cottage and holiday home, near Yattendon, a picturesque place—

> "Among the woods and tillage
> That fringe the topmost downs"

of Berkshire, with other interesting literary and historical associations, and in 1907 he purchased it, and, after some alterations, made it his permanent home. For the first two or three years the retirement was only nominal, for, although the release from the cares and responsibilities of the University position was a great relief, his duties in connection with the various Royal Commissions and Committees on which he was still engaged necessitated much travelling and frequent absences from home. Nevertheless, he managed to obtain much enjoyment out of his country life.

He busied himself with local affairs, became a member of the Yattendon Parish Council, and of the Conservative and Unionist Association of South Berks, and "people's" churchwarden of Yattendon Parish, and was a member of the managing body of the village school. He took to horticulture and was keenly interested in his garden. He was fond, too, of country rambles and of the sights and sounds of the country-side, its hedge-rows and woods and birds. Indeed, without being a professed ornithologist, he had a remarkable knowledge of birds and their habits, and was a member of the British Ornithologists' Union. Armed with his Zeiss and a note-book he would spend hours in studying and recording their characteristics, and would tramp for miles on the off-chance of meeting some rare or occasional visitant, the sight of which, with the locality, date, and other particulars, he would chronicle with as much satisfaction as he would have noted a novel and first-rate physical observation. He was fond of sketching, amused himself with colour-photography and in astronomical observation. At one period of his life he collected English china, and it was characteristic of him that he once lectured on a method of showing how soft paste or "bone china" might be distinguished from hard paste or oriental porcelain by means of the Röntgen rays. He made occasional contributions to periodical literature and interested himself in Lady Rücker's work in connection with the movement for promoting University courses of instruction in Household and Social Science, and was one of the original trustees of the endowment raised for this purpose. He was of the opinion that the education of a woman which best fits her for the management and direction of a home is as capable of being placed on a University plane as any other technical study.

The magnetic survey of the British Isles occupied much of his holiday time in the spring and summer, from 1883 to 1892, but after 1893 he resumed his visits abroad, sometimes to the South of France and to Sicily, partly, it must be said, in consequence of peremptory injunctions to take rest.

In 1897 he visited Canada as a member of the British Association and took part in the trip across the continent. In 1905 Lady Rücker and he got together a party of friends on an Orient liner to see the total solar eclipse of that year in the Mediterranean. Other holidays were spent in Donegal, in Pembrokeshire, in Devonshire, and on the South Coast. These various trips were found to be increasingly necessary. He had been showing signs of physical exhaustion during the last seven years of his work at the Royal College of Science, and his ceaseless energy was beginning to tell upon a constitution never very robust. In 1895 and again in 1901 a serious breakdown, with some symptoms of the arterial degeneration which eventually proved fatal, necessitated three months' complete rest. After his retirement in 1908, these troubles increased, and in 1911 there was no doubt that his condition was very serious, and public work had to be steadily curtailed. He spent part of 1912 in Harrogate with some benefit. In the summer of 1913 he went with his wife and son to Alsace, where he had a paralytic seizure at Altenburg. He was brought home from Münster on November 1 and on the same day two years later—All Saints' Day—he passed peacefully away in his sleep, apparently without pain. He had borne his long illness, and the gradually increasing sense of weakness and weariness, uncomplainingly and patiently, and with a beautiful serenity of mind and a calmness of resignation that profoundly touched all who were brought in contact with him. He was buried in Yattendon Churchyard in the presence of a gathering which included representatives of nearly every corporation or institution with which he had been associated during the forty years of his public life.

Arthur Rücker was a singularly lovable character, and one of the most unselfish of men. His charm of manner, his ready sympathy and instinctive tact, combined with his transparent honesty and straightforwardness of purpose, were the real source of his power in dealing with men. He disarmed at once any possible doubts as to the integrity of his motives by the very manner in which he exposed them, and he gained adhesion simply by the unconscious exercise of a mind that knew no guile. This sympathy, combined with a real humility and an inexhaustible patience, made him sincerely anxious to appreciate what there was in "the other man's" point of view. He was a loyal colleague and a staunch friend; men indeed prized his friendship as much as they valued his counsel. An excellent travelling companion, not even the discomforts of cruising in a small yacht among the Western Isles, amidst persistent drizzle and heavy seas, with occasional shortness of food, could damp his imperturbable good humour or affect his cheerfulness. Indeed, a little spice of adventure seemed to call forth some of his best qualities. He was keenly alive to the joys of the open-air life, and very susceptible to the influence of natural scenery.

No sketch of Rücker's character would be adequate that failed to take note of his deep religious feeling. This side of his character never obtruded itself; but, nevertheless, it made itself felt even upon a very slight acquaintance. "His religion was a heritage," says Mrs. Arnold-Forster, "that he received from his parents; that he preserved throughout his life, but which he had added to, deepened, widened and enriched. It was literally the key-note to which his whole being was attuned, and it is impossible to think or to speak of him without recalling that dominating characteristic of his moral nature."

T. E. T.

GEORGE MINCHIN MINCHIN (1845–1914).

George Minchin Minchin was born at Valentia Island (Kerry), May 25, 1845. When he was about nine years old, his mother died, and he was taken to Dublin for its educational advantages, being much with his uncle-in-law, Mr. David Bell, a Shakespearean scholar and a schoolmaster. From Mr. Bell he probably derived some of his literary skill, but his mathematical ability soon attracted attention. In January, 1862, he entered Trinity College, Dublin, under Dr. Shaw as tutor. In 1865 he obtained a University scholarship in mathematics, A. W. Panton being second. In the same year he got the Lloyd Exhibition in mathematics, pure and applied. In 1866 he and Panton obtained gold medals in mathematics, and began to take an interest in experimental physics. In 1871 and 1872 he was awarded the Madden Premium instead of a Fellowship. He was elected a Fellow of the Royal Society in 1895, and he died March 23, 1914.

In the spring of 1875 he was appointed Professor of Mathematics at the Royal Indian Engineering College, Coopers Hill, the late Sir George Chesney being then president. In 1888, Sir George, having occasion to testify to Minchin's capabilities, wrote, among other things, as follows:—

"There is one point on which I may usefully testify from an experience of ten years as head of Coopers Hill, and that is to his remarkable gifts as a lecturer. He combines in a peculiar degree the power of lucid explanation, and of interesting his pupils in subjects which, under ordinary handling, may easily be made dry and repulsive. Over and over again when visiting his lecture room with the intention of staying only a few minutes, I have found myself sitting out the whole lecture, so interesting, and indeed charming, did I find his demonstrations on the black-board of the processes of both pure and applied mathematics."

By universal testimony his teaching was clear, broad, and enthusiastic, and he was most popular with the students, not only inside but also outside the lecture room. Among other claims to their liking, he was one of the best lawn tennis players in the College, and his extraordinary exhibition of energy and skill in the lawn tennis of those days, which consisted chiefly in long spells of lobbing from the back line, always attracted a gallery. He was in fact a keen athlete, and in his younger days played cricket for the Gentlemen of Ireland.

He is best known as a mathematician and writer of classical text-books in Mathematics, the most famous of which is his 'Statics,' which is a model of elegant scholarship and clearness of exposition, and upholds the best traditions of the great Dublin School. Alfred Lodge, one of Minchin's colleagues at Coopers Hill, says that, "His various mathematical papers exhibit the same qualities of clear accurate reasoning and elegance of exposition. He could never tolerate a slipshod argument or a carelessly written sentence. If any one came to him for assistance in a mechanical problem, he was always ready to work out the solution, apparently without effort, on any scrap of paper that was handy, but always with scrupulous neatness, and without either hurry or hesitation: he never seemed to have to search for a method or to consult a book."

But he was by no means solely a mathematician; his interest in experimental physics was always very keen; and the writer first made his acquaintance in or about the year 1875, when Minchin came to University College in his spare hours to work in the then comparatively new laboratory which Prof. Carey Foster had there started. He went through a certain course of training in physical measurements, but his chief interest lay in original investigation, and he was always trying experiments, both in the laboratory there, and at Coopers Hill. He was a bachelor then, and the writer often went to visit him at the College, spending the night in a room which had been cleared for the occasion by the removal or piling up of numerous bird cages; for he was a great bird lover and had a collection of small birds which, if not large, was more than sufficient for an ordinary bedroom.

He was an early riser, and his books were mostly written before breakfast. He was also a notable correspondent; and the letters he received from George F. Fitzgerald, like all else written by that remarkable man, together with Minchin's replies, ought to be of interest if made accessible.

His best known physical investigations were probably those on the effect of light on metal plates coated with specifically absorbent chemical compounds; he found that aniline and other highly coloured dyes were able to make platinum plates electrically responsive to light, and he succeeded in making them fairly sensitive. These experiments went on until he arrived at a kind of coherer effect in these cells, and developed them into what he called "impulsion cells," with which he afterwards detected Hertzian waves.

Another electrical investigation, in which he persuaded the writer to co-operate, had reference to the application of the properties of selenium to the electrical transmission of views and pictures. His idea was to use a multiple cable consisting of a great bundle of insulated wires in parallel, to coat the ends with a film of selenium, to throw a picture upon the film, and to cause the distant end to reproduce, by action on a photographic film, the luminous intensity corresponding to each wire, thus developing a reproduced picture as a kind of mosaic. The chief difficulty seemed to lie in obtaining a transparent conductor. Electrolytes were tried, but they introduced difficulties of their own, and it cannot be said that our efforts met with success.

A metrical invention about which he was enthusiastic, and in the development of which he must have spent a good deal, was an absolute sine-electrometer; that is to say, a kind of trap-door electrometer with a light aluminium trap-door hanging vertically and examined by a microscope; the guard and opposed plates, fixed together, being tilted by a micrometer screw until the trap-door, if electrified, came back to its original sighted position. It is no doubt a metrical variety of the gold-leaf electroscope, and it can be made extremely sensitive. Such an instrument has since been developed further by others, and is now in use, being made by the Cambridge Instrument

Company in a form called the Wilson-Kaye tilted gold-leaf electrometer; though I am not sure that the absolute voltage can be calculated from its dimensions, as Minchin intended.

These examples are given as some indication of the less known work of Minchin, for there is no need to emphasize the value of his mathematical text-books. An article on his photo-electric discoveries will be found in 'The Photographic News,' vol. 35, pp. 57 and 61, being the number for January 23, 1891. He exhibited occasionally at the Royal Society's Conversazioni. He wrote papers sometimes on general subjects, which were often full of humour: and he was not above composing humorous scientific poems. Sir George Greenhill says of Minchin: "I think 'genial' will express in a single word the impression of Minchin's nature. He dearly loved a scientific rally, and to take part in a discussion, where his remarks were put together with rhetorical skill."

To give a better idea of Minchin's enthusiasm for laboratory work, I have asked Commander Rollo Appleyard, R.N.V.R., to jot down some reminiscences of the years during which he acted as his voluntary assistant; and here follows an abbreviation of what he writes:—

The seven years, 1885–1892, during which I held a junior appointment in the department of physics at the Royal Indian Engineering College, Coopers Hill, brought me into constant association with him. He was the most senior, I the most junior of the educational staff; but, in our enthusiasm for experiment, such differences were forgotten.

From the top of Coopers Hill, the Physical Laboratory looked out across and along the Thames Valley—miles and miles. Physics was accorded there a lecture theatre, a preparation room, a main laboratory, and a small optical laboratory. Most of Minchin's experiments were carried out in the small room. This could be darkened by means of folding wooden shutters. There was a lawn and a garden of roses outside it, and through the garden was a path which led to the college woods and across Runnymead to the boat-house. You must picture him there early on a summer's morning, looking out sometimes to watch the blackbirds and the wagtails on the lawn. Upon a slate table, firmly built into a solid brick and cement foundation, is the Clifton electrometer. It was carefully dried and "charged" last night by his kindly host Prof. Stocker, just after the last student left the laboratory. The "spot" is as steady as a rock, for there is no mechanical vibration to disturb the laboratory table at Coopers Hill. Now a warning bell informs the professor that a class of students is waiting for a lecture on Applied Mechanics. There begins a series of forced marches between the blackboard in his lecture room and the electrometer in the laboratory. Noon at last arrives. The paraffin lamp is blown out, the cap and gown are replaced by a straw hat and a suit of flannels, and away go the professor and his racquet to the lawn tennis courts. This is not because he is tired of his experiments, but because students are now coming in for their laboratory work. At about four o'clock they depart, and the professor and his racquet return, until dinner time. Briefly, such was his life, day by day, year by year.

He had at last produced an impulsion cell which was exceptionally sensitive to light, and he had for some time known that the sensitiveness varied in a capricious manner when an electric gas-lighter was "sparked" near such a cell. Moreover, in the course of some experiments which Mr. W. G. Gregory was carrying out, in the preparation room, with an induction coil and a Hertz oscillator, which you had shown us how to make, it was found that the sensitiveness of the photo-electric cell was affected, although several thick walls intervened. Then we rigged up an oscillator on the lawn in the garden.

upon a table, with an induction coil connected to it, and this apparatus we took back step by step across the lawn up to the edge of the wood, and the cell responded, though weakly. It was regarded by us as a most uncanny phenomenon. Upon this occasion Prof. Minchin used a horizontal antenna.

Later we tried to make the electrometer close an electric light switch and we succeeded.

The task he set himself to perform in the laboratory was to examine a vast series of substances for their photo-electric effects, particularly with regard to E.M.F. He was aware of the discovery by Willoughby Smith of the effect of light in altering resistance. He knew also what Shelford Bidwell and Becquerel had done ; but he was intent upon something even more subtle. Those of us who worked with him and beside him, urged him to try apparatus upon a large scale with a view to a result of practical utility. This we could not persuade him to do. His was the higher motive. With extreme patience, year after year, he continued the same kind of research, with no thought other than the advance of truth.

Minchin applied his photo-electric cells to measure the light of stars, and, in a paper read before the Royal Society in 1895, he communicated some interesting and promising results which he had obtained at Mr. Wilson's observatory, Daramona, using a 2-ft. reflector in conjunction with a specially arranged photo-electric cell. The relative magnitudes of Regulus, Arcturus, and Procyon, as thus measured, were found to be in good agreement with those determined by other photometric methods. When Coopers Hill College closed he moved to Oxford, for the sake of the telescopes and laboratories, and there to the last, making use of Prof. Townsend's laboratory, he worked at the physical problems which were the deepest interests of his life.

Concerning his mathematical work at Oxford, Prof. E. B. Elliott says—" His communications to the Oxford Mathematical and Physical Society were of the nature of instructive lectures on the behaviour of the functions of analysis—elliptic integrals for instance, and Bessel's functions —illustrated by very carefully prepared graphs. He was a very interesting conversationalist—full of ideas on all manner of subjects, with strong feelings and an Irishman's fire under restraint. His methods of work must have been most painstaking. In reading he seemed to accept nothing without making it his own by thought and independent investigation. I have his copy of Jacobi's 'Fundamenta Nova,' and it is full of his own manuscript. When he studied it (probably in early days) he must have gone on the principle of learning the subject by doing everything over again for himself in his own way. His patience must have equalled his keenness. To trust in authority and accept a conclusion without himself reasoning up to it was abhorrent to him."

Minchin's lectures often flashed with Irish wit, even his sarcasms being good-humoured and enjoyable. His students were enthusiastic about his personality, and one of them wrote thus in the 'Pioneer Mail' of India, April, 1914:—" Every Coopers Hill man in India will see with regret the news of the death of Prof. George Minchin, F.R.S., for those who did not know him personally must have known him by reputation. Mathematician, man of imagination, inventor, an admirable lecturer with a natural turn for the elucidation of difficult subjects, added to the soul of a poet and the heart of a boy—his was indeed an extraordinarily gifted and many-sided nature. To the outside residents of Egham Mr. Minchin may merely have appeared as the most enthusiastic lawn-tennis player and cricketer about the College. The newly joined Professor would probably gather that his peculiar sphere was the common room. Few would guess at first sight that behind his bonhomie lay a heart-whole devotion to science and knowledge . . . . His friends will cherish the memory of a singularly lovable man ; and there must be many in this country who would acknowledge that they came out to India stronger in mind and spirit for having come under the influence of his fine teaching and unobtrusive example."

To sum up: Minchin was an able mathematician, a brilliant teacher, and a genial comrade.

O. J. L.

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